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LORD KELVIN

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LORD KELVIN

BY

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PREFACE

LORD KELVIN has been called the Admirable Crichton of the Physical Societies; for in addition to being a great physicist, he was a great mathematician and a great engineer. During the seventy years he worked at these subjects, he made many great scientific discoveries and applied some of them with marvellous skill to the perfecting of inventions which have enabled mankind to use natural forces as their servant. Their applications are continually extending and have saved countless people from much monotonous labour and drudgery. Kelvin took a leading part in public life, more especially in connexion with education. Sir J. J. Thomson has said of him: "Never was so great a physicist, so great an engineer."

It would be impossible for me to give more than a slight sketch of his work and his influence on his contemporaries. But as one who was educated under him at Glasgow and who subsequently at Cambridge attended the lectures of several of his friends and contemporaries, I know the repute in which he was held by great scientists and so venture to give a brief account of his life and work.

I have to thank the Councils of the Royal Society and the Institution of Electrical Engineers for granting me permission to make use of papers in their Journals. I am deeply indebted to Agnes Gardner King for her biography entitled *Kelvin the Man*, in which she gives an excellent account of her uncle's personal life and char-

acter. Other biographies to which I am indebted are —the standard biography, *The Life of Kelvin*, by Silvanus Thompson, and that by Kelvin's assistant and successor, the late Professor Andrew Gray of Glasgow. Much information as to Lord Kelvin's immense help to navigation is contained in the second Kelvin lecture delivered to the Institution of Electrical Engineers by Prof. J. A. Ewing (Sir Alfred Ewing). I have to thank Messrs. Macmillan and Co. for kindly allowing me to make use of Thompson's biography and Messrs. Hodder and Stoughton for kindly allowing me to make a few extracts from *Kelvin the Man*; also the councils of the Royal Society, and of the Royal Society of Edinburgh, for permission to make use of their publications.

A. R.

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Chapter I

PARENTAGE. GLASGOW COLLEGE BEFORE 1871

LORD KELVIN'S father, James Thomson, a very able man who showed great talent at an early age, was born at Annachmore, near Ballynahinch, County Down, Ireland, in 1786. Kelvin's grandfather was a farmer whose ancestors came over from Scotland in the days of the persecutions of Claverhouse, between 1677 and 1681. The old farmhouse, Annachmore, still exists. The Thomson family remained in possession of it until 1847. James Thomson, when twelve years old, was an eye-witness of some of the horrors of the Irish Rebellion of 1798 and these terrible scenes left an ineffaceable impression on his mind.

The fact that James, when little more than a child, discovered for himself the art of making a sundial, showed that his ability must have been of a high order. He made sundials of slate both for the horizontal and vertical positions, and these were in the possession of his grandson James Thomson, when a shipbuilder at Newcastle-on-Tyne. At school James Thomson's progress was rapid and successful. At the end of his course, he was offered and accepted the post of assistant teacher. At this period his ambition was to become a Presbyterian minister, but, with the approval of his parents, he resolved to remain as an assistant school-

master while he thriftily saved the requisite money to pay for his college education.

It was not until 1810, when he was twenty-four, that he was able to enter Glasgow University as a student. In those days it was a long and trying journey from County Down to Glasgow, and the small vessels which sailed between Belfast and Glasgow were often delayed by storms, or in the autumn were becalmed for days at a time in the Firth of Clyde. Lord Kelvin used to tell how a smack laden with a cargo of lime, in which his father when a student was returning to Glasgow University after the summer vacation, was becalmed near Ailsa Craig and was carried three times by the tide round this rocky islet, about two miles in circumference, before enough wind arose to enable the voyage to be continued. On another occasion, he and some fellow-students, fearing that they were pressed for time, asked the captain to land them on the coast of Ayrshire, so that they might walk to Glasgow—a distance of between thirty and forty miles.

At Glasgow University James Thomson won prizes in classics, mathematics and natural philosophy, and took the M.A. degree in 1812. Afterwards, he attended all the theological classes and most of the medical, this being then a not uncommon practice amongst Scottish students. As the winter session lasted only six months, students were able to take some post in the summer and so obtain money to pay, either in whole or in part, the small college fees and the necessary living expenses during the winter. Some of them acted as tutors, helping the sons of well-to-do parents with their school or college studies.

Thomson's success as a mathematical teacher and the

high honours he gained at the university led him to abandon the idea of entering the ministry. In 1815, the year of the battle of Waterloo, he was appointed Professor of Mathematics in the Royal Academical Institution of Belfast, a position which he held for seventeen years. During this period he acquired a great reputation both as a teacher and as a clear and accurate writer. His treatise on *Arithmetic*, published in 1819, offers convincing testimony to the lucidity of his explanations and his wide knowledge of the subject. The examples have been chosen with great care, most of them being excellent illustrations of the application of arithmetic to commercial and scientific problems. Subjects now usually relegated to textbooks of algebra are also to be found in the book—for example, the theory of continued fractions. The ingenious method of extracting square roots by the method of continued fractions is explained, though the method had only recently been discovered by the French. References are freely given to eminent mathematicians like Lacroix and Legendre. Altogether, the book is of a very different type from the school books now in use.

That the book was a success is proved by the fact that it ran through seventy editions in sixty years. It had a great influence on the methods of teaching adopted in this country. In 1880 the author's sons, Professors James Thomson and Sir William Thomson, edited the seventy-second edition.

Amongst other books he wrote when at Belfast are treatises on Geometry, Geography, Astronomy and the Calculus. They all give a thoughtful and logical development of the subjects discussed and contain many carefully worded definitions. In after years his eldest son,

Professor James Thomson, showed a similar love for inventing definitions characterized by minute accuracy of statement.

In 1829 the University of Glasgow conferred on James Thomson the honorary degree of LL.D., and in 1832 he was appointed, by the University [redacted]-fessor of Mathematics to the University. When at Belfast he had married Miss Margaret Gardner, the daughter of a Glasgow merchant. They had a family of four sons and three daughters. His second son, William, the future Lord Kelvin, was born on 26th June, 1824. Lord Kelvin's niece, Agnes Gardner King, gives interesting details of his early life in her book *Kelvin the Man*, and tells us that he had a "loving and lovable" nature; an artist who asked permission to paint his portrait painted him as a cherub. The children were born in a comfortable house built by their father in what was then the outskirts of Belfast.

Whilst at Belfast, Professor Thomson lost both his wife and his youngest daughter. The blow was a heavy one, but he obtained partial consolation by devoting himself whole-heartedly to the education of his young family. His eldest daughter, Elizabeth, wrote a book entitled *Lord Kelvin's Early Home*, in which she gives her recollections of her mother. Anna, James's second daughter, married William Bottomley, a prosperous merchant of Belfast who had literary and scientific tastes and took an active part in public life. Her eldest son, James Thomson Bottomley, went to Glasgow in 1870, as a private assistant to his uncle, Sir William Thomson, and was a great help to him in both his teaching and consulting work (see p. 141).

Professor Thomson's eldest son James was ten years

old when the family moved to Glasgow, and even then was showing great inventive ability. William, the future Lord Kelvin, was about two years younger. James succeeded Professor Macquorn Rankine in the Chair of Engineering at Glasgow University. The two younger sons were John and Robert. John, born in 1826, took up medicine and became resident physician at the Glasgow Royal Infirmary, where he died of a fever caught in the discharge of his duties. Robert, who was born in 1829, emigrated to Australia, where he married and had three daughters.

When Professor Thomson moved to Glasgow his wife's sister, Mrs. Gall, kept house for him. He was an admirable father, and although a strict disciplinarian, never alienated the affection of his children. The education of the elder ones he superintended personally. In after years Lord Kelvin used frequently to say that all that he had learned as a boy in English, geography, history, mathematics and classics was taught him at home, along with his brothers and sisters, by their father. He used to add that he never met a better teacher in anything, than his father was in everything.

When the Thomson family arrived at Glasgow College in 1832, there were already three Professor Thomsons in the University. Professor Thomas Thomson was the Professor of Chemistry. He was the first to establish a chemical laboratory for students in this country, his laboratory preceding that of Liebig at Giessen by several years. In addition, there was William Thomson, the professor of Materia Medica, and Allen Thomson, his brother, who was Professor of Anatomy.

It is difficult nowadays to imagine what life in the University of Glasgow was like a hundred years ago.

These were the days of wooden ships, sixpenny newspapers and shilling postage, the days of flint and steel, of the Brown Bess, the three-decker, and the press-gang; the battles of Waterloo and Trafalgar were still living memories.

The University at that time had its home in the High Street. Even in 1871, after it had been moved to its present position on the top of Gilmorehill, its fine old gateway and part of one of the courts still adorned that somewhat squalid street. A university which for four hundred years has seen the tide of human life flow round it is full of memories which the demolition of its walls can only partially destroy. In the eighteenth century the Provosts and the Bailies still dwelt in the High Street and its continuation, the Saltmarket; and the houses of the tobacco lords and the West India merchants in Virginia Street were not far off. A hundred years ago the great migration of the civic statesmen and merchant princes of Glasgow towards the west end of the city had begun. The old college is now a railway station and no vestige has been left of the buildings to serve as a memorial of the old days. It was here that Lord Kelvin grew up until he was seventeen, when he left Glasgow for Cambridge.

On the occasion of the presentation of his portrait to Professor G. G. Ramsay, in 1907, the recipient gave a vivid picture of the High Street and College in the old days. "There was something in the very disamenities of the old place that created a bond of fellowship among those who lived and worked there, and that makes all old students to this day look back to it with a sort of family pride and reverence. The grimy, dingy, low-roofed rooms; the narrow picturesque courts,

buzzing with human life; the dismal foggy mornings and the perpetual gas; the sudden passage from the brawling, huckstering High Street into the academic quietude, or the still more academic hubbub of these quaint cloisters, into which the policeman, so busy outside, was never allowed to enter; the tinkling of the 'angry bell' that made the students hurry along to the door which was closed the moment it stopped; the roar and flare of the Saturday nights, with the cries of carouse or incipient murder, which would rise into our quiet rooms from the Vennel or the Havannah; the exhausted lassitude of the Sunday mornings, when poor slipshod creatures might be seen, as soon as the street was clear of church-goers, sneaking over to the chemist's for a dose of laudanum to ease off the debauch of yesterday; the conversations one would have after breakfast, with the old ladies on the other side of the Vennel, not twenty feet from one's breakfast table, who divided the day between smoking short cutty pipes and drinking poisonous black tea—these sharp contrasts bound together the college folk and the college students, making them feel at once part of the veritable populace of the city, and also hedged off from it by separate pursuits and interests."

At the early age of ten, William matriculated at Glasgow University, and he and his elder brother James went through the arts classes together, the younger brother always coming out higher up the list, but with the elder a good second. William's quick perception and wide knowledge astonished his class-mates. The prizes were not always given on examination results; some of them, in accordance with an ancient custom, were given by the votes of the students in the

class. In many cases this worked remarkably well; the results of viva voce examination, essays, and written exercises helping the students to pick out even quiet and unassuming men who had ability in particular subjects. Many times the popular vote gave the same order as that determined by the written examinations.

The University Calendar for that period gives some idea of the subjects lectured on by the professors. For example, in order to get the highest distinction in mathematics in the degree examinations, the candidate must profess Lagrange's *Theory of Functions* and *The Analytical Works of Apollonius and the other Ancient Geometricians*. In natural philosophy the whole of Newton's *Principia* and Laplace's *Mécanique Céleste* must be known. The calendar also makes the alarming statement that candidates must answer all the questions set with perfect accuracy.

In 1835–6 William received a prize for a vacation exercise, a translation of Lucian's *Dialogues of the Gods*.

In 1836–7 and 1837–8 the brothers were in the junior and senior mathematical classes and in each year William was first and James was second. In the second of these years, William appears as a second prizeman in the logic class, while James was third and John Caird, afterwards a famous preacher and principal of the University, was fifth. William and James took the first and second prizes in logic in 1838–9; and in that year William gained the prize in astronomy and a university medal for an essay on the figure of the earth. In 1840–1 he appears as fifth prizeman in the senior humanity (Latin) class.

In his address on his installation as chancellor of the University of Glasgow in November, 1904, he said:

"To this day I look back to William Ramsay's lectures on Roman antiquities, and readings of Juvenal and Plautus as more interesting than many a good stage play I have seen in the theatre.

"Greek under Sir Daniel Sandford and Lushington, logic under Robert Buchanan, moral philosophy under William Fleming, natural philosophy and astronomy under John Pringle Nichol, chemistry under Thomas Thomson, a very advanced teacher and investigator, natural history under William Cowper, were, as I can testify by my experience, all made interesting and valuable to the students of Glasgow University in the thirties and forties of the nineteenth century. . . .

"My predecessor in the natural philosophy chair, Dr. Meikleham, taught his students reverence for the great French mathematicians Legendre, Lagrange, and Laplace. His immediate successor in the teaching of the natural philosophy class, Dr. Nichol, added Fresnel and Fourier to this list of scientific nobles: and by his own inspiring enthusiasm for the great French school of mathematical physics, continually manifested in his experimental and theoretical teaching of the wave theory of light and of practical astronomy, he largely promoted scientific study and thorough appreciation of science in the University of Glasgow. . . ."

The Thomsons lived in the official residence of the mathematical professor, which was in the old college in the High Street. The surroundings, except on the site of the College Green, were somewhat squalid. The thick fogs which occurred in the winter time in the neighbourhood of the Old College often made it necessary to have the gas burning indoors all day. This and the cold east winds in the spring made climatic conditions

very unfavourable, but the long summer holiday (six months) gave the Professor and his family a welcome and ample opportunity for recuperating their health. For example, in 1834, he arranged with the captain of the *Glenalbin*, a small steamer trading between Glasgow and Londonderry, to take him and his family to Invercloy (Brodick), in Arran, a mountainous and very picturesque island in the Firth of Clyde. At that time Arran was very sparsely populated. The Duke of Hamilton, who owned practically the whole island, would not allow his tenants to enlarge their cottages in order to make them attractive to summer visitors. But he could not prevent visitors from coming and living in those little thatched-roof cottages now, alas, all slate roofed. The primitive arrangements and the difficulties experienced in obtaining even the necessities of life added much to the zest of the holiday. The only bread obtainable was brought from Saltcoats, on the mainland, by a small sailing vessel, which arrived, weather permitting, twice a week.

It can be readily understood that the children revelled in this freedom from restraint, and that the excitement of exploring glens and hills, and visiting waterfalls, kept them constantly in the open air. The explorations were real adventures too: except for the curlews and the plovers they had the hills for a playground practically to themselves. Elizabeth, the eldest daughter, tells that on one occasion she was so ill when she left Glasgow that she had to be carried on board the steamer, and yet so great was the recuperative power of the Arran air that in a week or two she could go for long walks with her brothers.

The encyclopædic knowledge of Professor Thomson

naturally led him to take a great interest in the geology of Arran, which has now for three or more generations been a place of pilgrimage for geologists from all parts of the world. The northern part of the island is of volcanic origin and is exceedingly precipitous. The lofty precipices and deep ravines within easy reach of Invercloy must have aroused the curiosity of the children, and led them to ask many questions of their father. It is easy to understand the fascination which problems connected with Plutonic action and the physics of the earth's crust always had for his son William in after years.

James even at this early age showed considerable ability both as an inventor and an engineer. He made a model boat which the children christened the *St. Patrick*. From this period, doubtless, dates William's lifelong love for the sea and for sailors. The following passage from the great treatise on natural philosophy which he wrote in conjunction with Professor Tait is an illustration of the observations of childhood being turned to account in later life. It was probably written by Thomson.

"That the course of a symmetrical square-rigged ship sailing in the direction of the wind with the rudder amidships is unstable, and can only be kept by manipulating the rudder to check infinitesimal deviations; and that a child's toy-boat, whether square-rigged or 'fore-and-aft-rigged', cannot be got to sail permanently before the wind by any permanent adjustment of rudder and sails, and that (without a wind vane, or a weighted tiller, acting on the rudder to do the part of a steersman) it always, after running a few yards before the wind, turns round till nearly in a direction perpen-

dicular to the wind (either 'jibing' first, or 'luffing' without jibing if it is a cutter or a schooner)." This sounds as if it were the conclusion James and William came to when they were discussing why the *St. Patrick* would not sail steadily with the wind. In later years William's sailing yacht the *Lalla Rookh,* 126 tons, was well known to yachtsmen on the Clyde and in the Solent. He took long voyages in it, sometimes as far as Madeira, and, as we shall see, gained a reputation as an expert in navigation.

Amongst the many friends Kelvin made among his fellow-students at Glasgow was Francis Sandford, son of Sir Daniel Sandford, the Professor of Greek. Francis gained a Snell exhibition tenable at Balliol College, Oxford, and afterwards was well known as a great educationist. As Lord Sandford of Sandford he was one of Kelvin's sponsors when he entered the House of Lords.

In 1840, Professor James Thomson with some of his family made a tour in Germany as far as Bonn and Frankfort, his idea being that they might acquire some knowledge of German and have a holiday at the same time. Before they left, however, William had come across Fourier's treatise on the Conduction of Heat, and was so interested in it that he slipped it in with the luggage and surreptitiously studied it. He concluded that the strictures passed by Professor Kelland of Edinburgh on some of Fourier's theorems were unjustified, and when he confessed to his father his secret study and the conclusions at which he had arrived, he was not severely criticized. His father advised him to write to Kelland, very politely pointing out the error in his criticism. This he did, and at the end of the correspondence Kelland acknowledged that Thomson was right.

During all his life William Thomson talked and wrote about the "transcendent interest and perennial importance" of the solutions Fourier had obtained, and of their usefulness in nearly every branch of physical science. Some of his most important theoretical and practical work was done with their help. Even in 1907, the year of his death, he was busy applying these solutions to investigate the growth of a train of waves in water.

The reading of this book led him to write his first paper, which is headed "Frankfort, July, 1840, and Glasgow, 1841". In it he justifies Fourier's method against the strictures passed on it by Kelland in his *Theory of Heat* (1837), where Kelland says: "There can be little doubt to anyone who carefully examines the subject that nearly all M. Fourier's series in this branch of the subject are erroneous!"

His second paper, also dated 1841, discusses the cooling of a heated sphere in space.

In August of the same year he published an important paper, showing the equivalence of certain problems in heat and electricity. This was written in Arran a few months before he left with his father for Cambridge.

Chapter II

CAMBRIDGE

DURING the nineteenth century the Cambridge school of mathematics attracted students from all parts of the world and a good position in the Tripos was the hall-mark of mathematical attainments. A Glasgow student—Archibald Smith of Jordanhill—was senior wrangler and first Smith's prizeman in 1836. It was not surprising, therefore, that Professor James Thomson encouraged his son William to go to Cambridge, where it was certain that he would do well in his examinations. Dr. Thomson also had in view the professorship of natural philosophy at Glasgow University, the holder of which, Professor Meikleham, was getting on in years and was in precarious health. The possession of a brilliant Cambridge degree would be a great help to his son if he should ever apply for the post.

In 1841, before he had completed his seventeenth year, William Thomson was entered as a student of St. Peter's College, usually called Peterhouse, at Cambridge. He used to talk sometimes, in after years, of his first going to Cambridge in a mail-coach, accompanied by his father and mother, and a pleasant Swedish gentleman who made the fourth inside passenger. They started about 7 on a bright October morning and arrived at Carlisle in the evening, putting up at a com-

fortable hotel where they had a luxurious tea. Next morning they started again as inside passengers in the mail-coach for Hull, dining on the way, and reached that town before sunset. Mrs. Thomson was then put on the ferry-boat across the Humber, where she met a carriage which took her to the house of the friend with whom she meant to stay. Professor Thomson and William had intended to go by sea to London, but as they did not like the look of the berths and cabins, they took the mail-coach again all the way to Cambridge, which they reached without adventure.

Travelling in 1841 was very different from what it is to-day. Crowds would collect to gape at a steamboat or a steam locomotive. On moonless nights men had to grope their way along narrow streets by the help of swinging lanterns or torches, and conical iron torch extinguishers were placed in front of most big Georgian mansions. It is true that William Murdoch had been awarded the Rumford medal by the Royal Society in 1808 for the discovery of gas lighting, but for many years afterwards its progress for domestic use and street lighting was very slow. Travelling was difficult and expensive and in many places the roads were so bad that the stage-coaches had to proceed with the greatest caution. In London as late as 1840 the clocks in both Houses of Parliament were regulated by the carriage of a chronometer from the Queen's private Observatory at Kew, and the inns often took their time from the watches of the drivers of the stage-coaches. When railways were first introduced, great confusion was caused by the difference between railway and local time. These were the days of the shilling post and the semaphore telegraph, and foreign news trickled through very tardily.

The semaphore line connecting the Admiralty with Portsmouth was out of action one day out of every three owing to poor visibility, and the news was garbled by the mistakes of the numerous operators. It was only about sixty years ago that mid-European time was first introduced into Germany, Italy and other countries, which fixed their time one hour in advance of Greenwich.

William Thomson was soon very popular with his fellow-students at Peterhouse, and was looked on as the coming " senior wrangler ". Some of the dons also who had noticed the paper he had written in the *Cambridge Mathematical Journal* were of the same opinion. Amongst his fellow undergraduates at other colleges he made several lifelong friends. One of these was Hugh Blackburn of Trinity, a native of Killearn, near Glasgow, who was fifth wrangler in Thomson's year, and was afterwards his colleague for many years as Professor of Mathematics at Glasgow. Another great friend was G. G. Stokes of Pembroke, who was senior wrangler just before Thomson came into residence. He was very proud also of his acquaintance with Archibald Smith, who encouraged him to proceed with his mathematical researches.

In his second year Thomson read privately with William Hopkins of St. Peter's College, whose ability as a mathematical coach was only equalled in after years by E. J. Routh of the same college. Hopkins was not the ordinary crammer who studies the idiosyncrasies of the examiners for the year and makes his students specialize in those subjects that will pay. He did not want them merely to limit their aspirations to mathematical honours, and strove to impart to them

a disinterested love of their studies. During his undergraduate course Thomson wrote no fewer than sixteen original papers, some of which are of great merit.

Hopkins was a remarkable man in many ways. He began life as a farmer, and did not enter as a student at Cambridge until his thirtieth year; graduating as seventh wrangler in 1827.

He became as distinguished in geology as in mathematics, and was president of the British Association in 1853.

Among his pupils, besides Thomson, were Stokes, Tait and Clerk Maxwell. In 1875, Tait wrote: "All of these . . . undoubtedly owe much (more perhaps than they can tell) to the late William Hopkins. He was indeed one whose memory will ever be cherished with filial affection by all who were fortunate enough to be his pupils."

Thomson was always careful to keep himself in perfect physical condition by taking the necessary amount of exercise. He liked swimming and rowing, and with Hemming of Trinity, who was senior wrangler in 1844, and afterwards became a distinguished Queen's Counsel, he went shares in a " funny "—a lightly-built pleasure boat with a pair of sculls—and practised rowing assiduously. He became an excellent oarsman and won the Colquhoun Silver Sculls. This was a great honour, as the prize is open to all undergraduates. Distinction in athletics, however, did not attract him overmuch, although he always enjoyed an out-of-door life. In a letter to his sister he said that the early mornings at Cambridge reminded him of the May mornings they used to enjoy in the Isle of Arran. In the summer months Thomson frequently went for a walk into

the country round Cambridge with one of his friends. He sometimes bathed in a pool in the upper Cam, well-known to undergraduates as Byron's pool. It is in the middle of a cowslip-covered meadow, with water-lilies growing near the banks, and is a favourite spot for good swimmers, who take a run on the grass and then a flying plunge into the pool.

In his last undergraduate year the shadow of the coming Tripos began to affect the pleasure of his existence. He knew that he had an excellent chance of being senior wrangler, but the chapter of accidents has always to be considered, especially in such a severe competitive examination. A bad headache or the expenditure of too much time over a knotty problem might upset his chances. Knowing his father's hopes and desiring to do everything he could to please him, he was bound to feel anxious. At the same time he was brimming with ideas, destined soon to give an enormous impetus to science, but of little help to him during the examinations.

When the Tripos list was published, and Parkinson of St. John's was declared senior wrangler, with Thomson second, he took his defeat philosophically. At the Smith's prize examinations their relative positions were reversed, Thomson being first. The papers set in this examination were admirably fair, and might be set to mathematical physicists of the present day. In one of the questions in Earnshaw's paper the candidates were asked to give a physical analogy between fluid motion, the attraction of bodies and temperature. This might have been suggested by one of Thomson's own papers which had only recently been published. Other questions about elastic solids and waves in canals must

have directly appealed to Thomson; in after years he enjoyed discussing questions of this type in his senior mathematical class.

During the years he spent as a student in Cambridge, Thomson indulged in his love for music, and he was one of the founders of the Cambridge University Musical Society. This started as a Peterhouse Society in 1843, after a first concert which we are told in a later *Cambridge Chronicle* was followed by a supper and by "certain operations on the chapel roof". The second concert was held at the Red Lion Hotel at Cambridge, as there was no suitable room in the college. The master would only grant his permission on condition that the society called itself the University Musical Society. This was formed in May, 1844, and the first three presidents were Smith, Blow and Thomson. The musical instruments that Thomson played were the cornet and the French horn, and in the original Peterhouse band he was the second horn. There is no record, however, of the difficulties he must have found in practising. Many years afterwards, when lecturing on sound to his students, he discussed the vibrations of columns of air in wind instruments. He sometimes illustrated his remarks by showing how the pitch of notes could be altered in an old-fashioned French horn, played with the hand in the bell, a performance which always greatly delighted his class.

At the Jubilee commemoration of the society in 1893, Kelvin stated that Mendelssohn, Weber and Beethoven were the "gods" of the infant association. Those of his students who came more intimately into association with him will remember his keen admiration for these and other great composers, especially Bach, and his

delight in hearing their works. The Waldstein sonata was a special favourite of his. It has been remarked that the music of Bach and Beethoven has special attractions for mathematicians.

Canon Wordsworth gives an amusing description of the origin of the word " tripos ". In the days long before written examinations, the senior bachelor had to sit upon a three-legged stool before the proctors. This three-legged stool was the only tripos at this period. Later on the bachelor was called the " tripos ", just as judges are sometimes called the " bench ". Subsequently the name was given to tripos speeches, then to tripos verses, and finally to the tripos lists.

After his examinations were over, Hopkins presented Thomson with two copies of an essay, written by George Green, on the mathematical theory of electricity and magnetism, which was published by private subscription at Nottingham in 1828. Thomson learned from it that the very important theorem in attraction which he had published in 1842 had been anticipated by Green. It is extraordinary how few people at Cambridge or elsewhere seem to have been aware of the existence of this essay, but the facts of Green's life partly explain it. His father was a miller possessed of private means, who lived at Sneinton, near Nottingham. The son was an entirely self-educated mathematician. When thirty-five years old he published his essay which shows thorough familiarity with the writings of the French mathematicians, more especially with Poisson's work. In 1833, Murphy, a tutor of Caius, published a small treatise on electricity, in which he mentions Green as the originator of the term potential, but he gives no reference to his theorem. At the age of forty, Green,

probably attracted by Murphy's reputation, entered Caius College, and graduated as fourth wrangler in 1835. He did not mix much with the students, and entered little into the life of the college. He was elected to a fellowship and died two years afterwards. Thomson used his influence successfully to get the importance of Green's papers recognized, and they have been published by Caius College in a volume edited by N. M. Ferrers, a senior wrangler, who took no inconsiderable part in advancing our electrical knowledge on the lines laid down by Green and Murphy.

In the early part of 1845, Thomson made the acquaintance of Michael Faraday and visited his laboratory at the Royal Institution. He was very proud of this acquaintance and used to show his class the piece of heavy glass which Faraday used at his first demonstration of the connexion between light and electricity, and which he presented to Thomson, who treasured it as one of his choicest possessions.

The Smith's prize result was published on 24th January, 1845. Two days later, Thomson, accompanied by his friend Blackburn, set out for Paris, where he was to spend some months. He was fortified with introductions to many of the most celebrated scientists in Paris. For some of these he was indebted to his Cambridge friends, among them Arthur Cayley, destined to become the most outstanding British mathematician of the century. Sir David Brewster gave him letters to Arago, Biot and Babinet, the leading French physicists of the day. A family friend, Walter Crum, F.R.S., of Thornliebank, contributed an introduction to Dumas, the famous chemist.

Among mathematicians, Laplace, Lagrange, Poisson,

and Thomson's great hero, Fourier, had by this time passed away, but there was still Cauchy, perhaps the greatest of them all in respect of influence on modern pure mathematics. To Cauchy he had a letter from Kelland, and another from J. D. Forbes, at that time professor of natural philosophy in Edinburgh, and afterwards Principal of St. Andrews University. Forbes, although a great friend of the Thomsons, was regarded with somewhat mixed feelings by Professor James, who thought he was likely to be a candidate, and an extremely formidable one, for the Glasgow natural philosophy chair when it should fall vacant. The fear, perhaps, was father to the thought; at any rate when the time came Forbes did not offer himself.

The first time Thomson called, Cauchy asked him how far he had gone in mathematics: did he know anything, for example, about the differential calculus? But he soon found out the young student's calibre, and they had many a talk, chiefly about what Cauchy himself had done and was doing. Cauchy was a keen Catholic, and tried to convert the young Ulsterman, with what success it is needless to say.

Besides Cauchy, there were also in Paris Chasles, the great geometrician; Sturm, familiar name to students of algebraic and differential equations; and, above all, Liouville, with whom Thomson came to be on really intimate terms. Liouville is important in the history of mathematics, both pure and applied, not only for his personal achievements, but also as the founder of the great mathematical journal that bears his name, a name strangely absent from English works of reference!

Hopkins had presented Thomson with *two* copies of Green's essay, one of them being intended for Liouville,

and Thomson now handed it over. Liouville was as excited over it as Thomson himself had been, and lost no time in displaying his prize to his mathematical friends, to whom Green's work was quite unknown. Some of them, in fact—Sturm, for example—had discovered some of Green's theorems for themselves, years after the publication of the essay. It is difficult not to feel disappointed when you find that some result on whose discovery you have been priding yourself is not really new after all, and this is what happened here. A few weeks after Thomson and Blackburn were installed in their attic, a visitor rushed in. This was Sturm, who in great excitement asked for the essay, of which Liouville had told him. Hastily turning over the pages, he soon caught sight of what he was looking for —his cherished formula, down in print, more fully and clearly than he had expressed it himself.

The famous physicist Biot was now an old man, and told Thomson that their best man was a young professor at the Collège de France, called Regnault, to whom he promised to introduce him at the next meeting of the Institute. This he did, and it was there and then arranged that Thomson should spend some hours daily during his stay in Paris, assisting Regnault in his laboratory.

The assistance was really of a very humble sort, holding a tube, working the air pump, and so on. There was nothing at that time, in Paris or anywhere else, corresponding to the organized teaching of students that is part of the regular routine of physical laboratories at the present day. For all that, Thomson's experience with Regnault was of the utmost value to him. Apart from its bearing on contingencies in Glasgow—the prospective

chair was never far from his father's mind—the experience of seeing a great experimenter at work, and of coming into daily contact with what was probably the finest collection of physical apparatus in existence, was of inestimable benefit to Thomson, who after all was as yet a mathematician rather than a physicist.

Regnault was engaged at that time in making his classical determinations of the constants in the theory of heat. The devices he used to secure accuracy and eliminate sources of error were much appreciated by Thomson.

He used when lecturing to dwell on Regnault's skill as an experimenter. He related once how mystified he and Blackburn were when they saw Regnault freezing mercury in a red-hot crucible, until it was explained to them that he was using the spheroidal state of liquid ether and the low temperature caused by its rapid evaporation.

In June, 1845, William Thomson was elected to a fellowship, worth £200 per annum, at Peterhouse. This was good news to his father who had sometimes upbraided him for his extravagance. The books he wanted to buy were expensive, and when he did buy them he liked to have them in good bindings.

In May, 1846, Professor Meikleham died, and the chair of natural philosophy in Glasgow University became vacant. Thomson's father had long been preparing for this event. He now wrote to Forbes of Edinburgh announcing the vacancy, and was delighted to learn that Forbes was not to be a candidate, but hoped that William would be, and would get the chair.

The electors were selected members of the Faculty of the University of Glasgow, and were the principal

and the professors of divinity, oriental languages, moral philosophy, mathematics, logic, Greek and humanity.

A difficulty the Thomsons had to contend with was the feeling in the minds of some of the electors that Oxford and Cambridge men had not always been successes in Scottish chairs; for example, Kelland, professor of mathematics in Edinburgh, and Lushington, professor of Greek in Glasgow, had not quite adapted themselves to their posts, and were inclined to lecture over the heads of their students. Professor James Thomson urged his son to do everything in his power to meet this objection. About his high mathematical attainment there was no question; what was wanted was some evidence, in his testimonials if possible, of his skill in practical work, and his ability to impart elementary instruction.

Besides Thomson there were five candidates, apparently not very formidable, although two of them were already professors of natural philosophy. Archibald Smith of Jordanhill hesitated long, and would have had very strong local backing, but eventually, like some other dangerous opponents, he did not come forward.

Thomson had a tremendous array of testimonials. Think of these names: Hopkins, Whewell, Peacock, Ellis, Forbes, De Morgan, Cayley, Townsend, Sir William Rowan Hamilton, Walton, Boole, Sylvester, Stokes, Regnault, Liouville!

His college tutor Cookson, and his private tutor, Hopkins, both insisted on their conviction that Thomson would prove perfectly capable of adapting himself to the comprehension of any type of class. Hopkins referred also to another circumstance, likely to add to Thomson's efficiency in lecturing, namely, the amia-

bility of his character and the simplicity of his manners, qualities which had made him extremely popular with all classes of his acquaintance at Cambridge.

Several of the testimonials, Cayley's in particular, predict Thomson's future eminence in science.

Even up to the end, it was not at all certain how the election would go. University politics were very keen at the time, the chief bone of contention being the proposal to abolish tests in appointments to professorships. James Thomson was strongly in favour of abolition, and to that extent was by no means *persona grata* to the official party in the Faculty, chief among whom were Principal McFarlane, and Dr. William Fleming, the professor of moral philosophy, flippantly designated by his students as " Moral Will ", his colleague in the logic chair, Professor Robert Buchanan, being similarly distinguished as " Logic Bob ".

The eventful day arrived at last, and Thomson was elected unanimously—a great triumph for a lad of twenty-two. His father was overjoyed. William himself went over to Thornliebank in the evening to tell his cousins, the Crums, and in particular Margaret, the lady who was afterwards to be his wife.

A gloom was cast over the Thomson family later in the year by the death of his youngest brother John, then resident assistant at the Glasgow Royal Infirmary, who died of a fever contracted at the hospital.

Professor James Thomson did not live long after his son William was appointed to the professorship. In 1848-9 a terrible visitation of cholera plunged Glasgow into mourning. Epidemics of this disease were not infrequent at this period and the lugubrious bell of the dead-cart tolling through the night air was a familiar

sound, inviting people to bring out their dead. Hardly a house escaped the scourge, rich and poor alike falling victims, of whom Professor Thomson was one.

Professor William Thomson remained on in the parental home in the University, his aunt, Mrs. Gall, keeping house for him until he married in 1852.

Chapter III

PROFESSOR OF NATURAL PHILOSOPHY

ON November 3rd, 1846, Professor William Thomson gave his first lecture to the natural philosophy class at Glasgow. He had spent a considerable time preparing it, and had written it out in full. Owing to nervousness, he read it much too quickly and felt that it had not been a success. He was much depressed in consequence, but very soon afterwards his enthusiasm for his subject made him forget the trammels which his preconceived notions about lecturing had put on his delivery and he quickly developed a more natural and much improved style. For more than fifty years Thomson opened the session with this lecture, with the same manuscript in his hands, but he very rarely indeed got to the end!

Attendance at the natural philosophy lectures was compulsory for all students who wished to take an arts degree, and the scientific knowledge of the bulk of the class was consequently very limited. As a professor, Thomson was inclined to forget this, and was apt to address his class as if it were a learned society. Moreover, his habit of making digressions, of the sort which, in later days, so interested and amused audiences at the London Royal Institution, was rather trying to those students whose ambitions were bounded by the degree

examinations. But all of them were proud to belong to his class and felt that it was a great privilege to be the pupils of such an eminent philosopher. At a later period, in the seventies and eighties of last century, the Arts Faculty of Glasgow University had as professors several men of great distinction; in particular, Richard Claverhouse Jebb, professor of Greek, one of the four or five British classical scholars of the first rank, of any period; and Edward Caird, professor of moral philosophy, afterwards Master of Balliol, and one of the foremost exponents of the Hegelian philosophy. But Sir William Thomson, as he was then, was placed by the students on a pedestal higher than even these heroes.

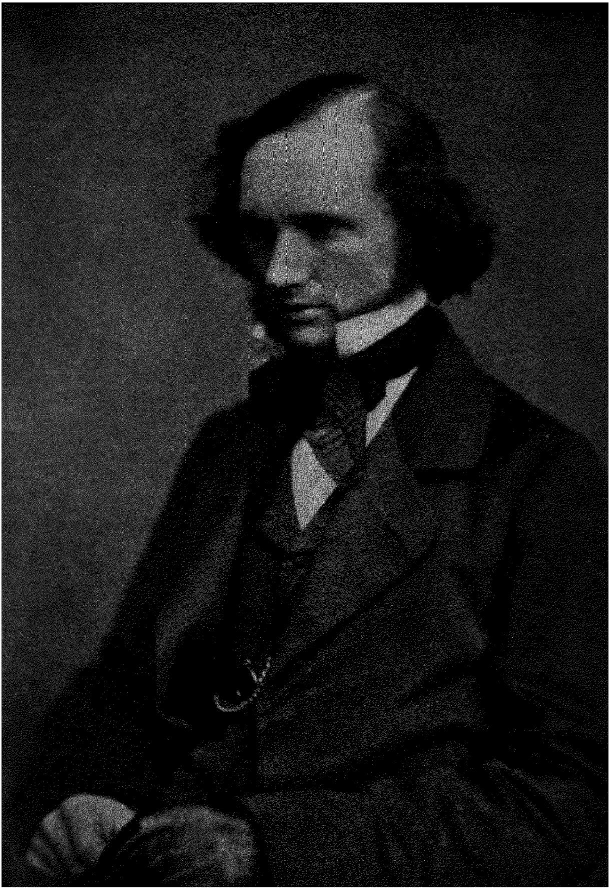
Small wonder, then, that for many of the students of the natural philosophy class the most interesting study was the professor himself. The late Principal Marshall Lang, of Aberdeen University, father of the present Archbishop of Canterbury, has said that "there could not fail to be interesting hours in *his* class. In 1850, he was in the vigour of youth, charming, with a face that shone, a figure lithe and graceful, distinction stamped on the personality."

Thomson opened his class every morning by saying, with his eyes shut, the third collect from the morning service of the Church of England. He then began his lecture, the students taking notes and looking with interest at the apparatus, sometimes very elaborate, set out for demonstrational purposes. As he was often deeply absorbed in physical problems, it was sometimes hard for him during lectures to keep his mind from straying to his own private difficulties. One felt that he was looking forward to renewing the attack on a problem when the lecture was over. In the senior class he went

even farther. Occasionally some problem would occur to him during the lecture. After a hasty résumé of it to the class, he would write down the equations on the blackboard and proceed to study them, the class meanwhile watching a great scientist attack an unsolved problem in mathematical physics. They saw him try one mathematical method after another in his attempts to wrest the secret from Nature and even those who could not follow his methods looked on with the greatest interest. But new discoveries are usually evolved very slowly, and when at the next lecture he told us the result, we could infer, judging by the progress he had made during class, that he must have expended many hours of hard thought on it in the interval.

Sometimes he had to report that the problem had already been solved by others. On one occasion, for example, when discussing the motion of gyrostats linked together, he discovered certain algebraical theorems in connexion with determinants, and asked us to verify them and try to generalize them when we got home. He finished the lecture in the highest spirits, but at the next meeting gave us a list of books in which he had found the theorem, and ended up by saying that it was even given in Todhunter's *Theory of Equations*. Todhunter was an excellent mathematician who was senior wrangler three years after Thomson's year, and was the author of numerous textbooks, mainly on mathematical subjects, which at that time were almost universally used. Possibly owing to their academic nature, Thomson had an antipathy to them.

He once in class asked a student what was the meaning of a symbolical expression written on the board. The student, being a good mathematician, replied with much



Professor William Thomson, 1852

complacency that it was the limiting value of the ratio of the increment of x to the increment of t when the latter increment was indefinitely diminished. His satisfaction was short-lived. Thomson's comment was: "That's what Todhunter would say. Does nobody know that it represents a velocity?" The general definition savoured too much of "cut and dried" mathematics—he wanted a physical meaning for the expression.

Many anecdotes, more or less authentic, have come down about Thomson's teaching. According to a story which has been often repeated, at one time he had an assistant called Day, whose prelections were a good deal more intelligible to the struggling members of the class than those of the professor. At the time Thomson received his knighthood, he was absent for some days, and the assistant had to take all the classes. One morning, when the class assembled, there was seen on the board the legend: "Work while it is yet Day, for the Knight cometh when no man can work." Unfortunately, we have Thomson's own authority for the statement that he never had an assistant called Day.

The oral examination in class was very frequently too much for the student concerned, who, quite unable to answer, stood dumb. Thomson would then urge the poor student to give some sort of answer, right or wrong. If it were wrong, it could be put right; but against *aphasia*, as he called it, he was helpless.

He advised us to look over a table of logarithms occasionally for amusement. Treated in the right way, he assured us that it would prove as interesting as a novel!

Some others of his favourite dicta were: "Dirt is

only matter in the wrong place"; "a little expenditure of chalk is a saving of brains"; "I never try to remember facts, but I do remember where I can get them when I want them"; "the art of reading mathematical books is judicious skipping"; "it is not until you have measured a physical quantity that you really can be said to know anything about it".

In his lectures on mechanics, he gave little that was of any value to a student cramming for his degree examination, but to the few who understood him his exposition of dynamical principles was most illuminating. The experiments, however, which he showed could be appreciated by every one. Many of these were of a type not to be seen anywhere else. He had a fine collection of massive gyrostats, which, in the days before motors, were set spinning with the help of a large fly-wheel. To show off the peculiar antics of the gyrostats was a source of perennial delight to our genial and enthusiastic professor.

He was less successful when it came to the explanation of the principles on which they worked. His regular formula was: "The principle of the gyrostat is perfectly simple; it is merely a matter of generation of moment of momentum perpendicular to the axis of the rotator." Then he would add: "A numerical exercise on this principle will be set in the paper on Monday morning." Needless to say, very few answers were handed in to this question.

An experiment which excited intense interest, not unmixed with hilarity, was one intended to illustrate the formation of a drop of water by the influence of surface tension. A brass funnel, two or three feet long and an inch wide, supported in a vertical position, had

a conical mouth at its lower end about a foot in diameter, across which a stout sheet of india-rubber was tightly and securely stretched. Various lines were drawn on the plane surface of the india-rubber, allowing the subsequent distortion of the sheet to be made evident. Water from the main was then carefully led into the funnel, and gradually the rubber bulged out till it took the shape of an immense water drop. The experiment, as the students knew, was always continued to bursting point, and the splash, when it came, was received with vociferous cheers, while the professor's face literally beamed.

Possibly of all Sir William's experiments the most highly appreciated was the one with a massive and ancient-looking piece of apparatus called Robins's Ballistic Pendulum. Benjamin Robins was a celebrated eighteenth-century engineer in the government service, who was the greatest authority of his time on ballistic science, and had invented this contrivance for the purpose of determining the velocity of a bullet. A heavy block of wood, weighted and strengthened by thick hoops of iron, was suspended like a child's swing from a cross-bar six feet or so above the ground. A measuring tape was attached to the block so that the deflection of the pendulum under the impact of the bullet could be measured. The bullet remained imbedded in the wood. When everything was ready, Sir William (as he was to his students in the eighties) took a rusty-looking rifle round to the front of the rostrum, adjusted his eyeglass, went down on one knee, took careful aim, and fired. Amid deafening applause the assistant rushed to take the length of tape drawn out, and wrote the figures down on the board for subsequent use in

the calculation. Sir William performed this experiment year after year, and never lost his delight in it. Sometimes at least, when the merriment was even more intense than usual, the class would demand an encore, which there is good authority for saying their revered professor, keenly appreciative of a joke and thoroughly understanding his class, did not always refuse.

Another experiment which excited rapt interest was one with ordinary hens' eggs. Sir William began by telling the story of how Columbus made an egg stand on end, to show how easy it was to do a thing—discover America, for example—when you had been shown the way. He said he was going to go one better than Columbus, and make an egg stand on end without breaking it. This he did with apparent ease, by spinning the egg rapidly with his fingers on the smooth tray on which it lay, whereupon the egg rose like a top and continued to spin round its long axis.

Sir William warned us, in case we ever tried the experiment for ourselves—as some of us certainly did—to see that the egg was *boiled.* When he applied the treatment to another egg, which was in its natural state, it refused to budge from the horizontal position. He also pointed out a still easier way of determining whether an egg was boiled or not. He set his two eggs rotating slowly, so that neither rose, and then put his finger for a moment on each. The boiled egg stopped dead, but the other, after a momentary rest, went on spinning as before. He explained that the liquid within the unboiled egg could not be brought to rest by merely touching the shell.

Thomson was most enthusiastic about the convenience of the French metrical system, and rarely let an oppor-

tunity pass of running down what he called the British " no system ". The people of this country, he would say, have for their unit of mass the grain, the scruple, the gunmaker's drachm, the apothecary's drachm, the ounce troy, the ounce avoirdupois, the pound troy, the pound avoirdupois, the stone (Imperial, Ayrshire, Lanarkshire, Dumbartonshire), the stone for hay, the stone for corn, the quarter (of a hundredweight), the quarter (of corn), the hundredweight, the ton, and several other units. This he contrasted with the beautifully simple French system. He thought it quite extraordinary that the British people, who pride themselves on their common sense, should condemn themselves to so much unnecessary hard labour. The strong prejudices of many engineers in favour of our system he regarded as a strange phenomenon, a matter for moral and social science rather than for physical.

Even in his introductory lectures Thomson soared to heights which made many of his class feel giddy and helpless. He would say, for example, that all motion is relative motion. We can calculate from astronomical data the direction in which and the velocity with which we are moving at any instant, by first compounding the known velocity of rotation of the earth round its axis with the motion round the sun. The resultant motion having been accurately determined, we have then to compound it with the roughly known velocity of the sun in space. But even if this were accurately known, it would not give us our absolute velocity in space, for it is only the sun's relative motion among the stars that we can observe. In all probability the sun, moon and stars are moving with inconceivably great velocities relative to other bodies in the universe. Having thus

unsettled the ideas of his class and awakened their interest, he would point out how easy it is to get the relative motion by the simple device of impressing upon all the moving bodies a velocity equal and opposite to the velocity of the one about which the relative motion is to be found.

On another occasion many of his class were brought to realize for the first time how extremely difficult it is to give a rigorous definition of what a second of time is. To say that it is a definite fraction of the period of the earth's rotation round its axis is only scientifically correct, provided that you give the date. Observations made on ancient eclipses, dating as far back as 720 B.C., make it highly probable that the period of the earth's rotation round its axis has lengthened by about the three-hundredth part of a second. As a timekeeper, therefore, the earth is not ideally perfect. Thomson stated that a carefully arranged metallic spring hermetically sealed in an exhausted glass vessel would be a more accurate measurer of time. Even in two thousand years tidal friction has quite an appreciable effect on the length of the day, and if we were legislating for fifty million years ahead, we should have to take into account the effects produced by the shrinking of the earth, due to its cooling.

When explaining physical principles, Thomson frequently made use of terms employed in navigation and astronomy. References to parallax and aberration, azimuthal and precessional motion, right ascension, fore and aft, and many other technical phrases made a large demand on the general knowledge of the class. A student once asked what he meant by the weather side of a ship. His reply that it was the side towards

the wind made the student feel that he had asked an unintelligent question. To remove this impression the professor explained that a ship is said to carry a weather helm when it is necessary to hold the helm on the weather side of its middle position to keep the ship on its course. This suggested that it would be useful to point out to the class that the natural tendency of a body moving in a liquid is to turn its length across the direction of its motion; that is why an elongated rifle bullet requires rapid rotation about its axis to keep its point foremost.

Towards the end of the session, owing to the very comprehensive programme that had to be got through, the pace had to be quickened. The last day was always an eventful one, the professor sometimes lecturing and showing experiments to those of the class who could remain, long after the hour was up. The author was one of those who remained to the end—a period of over four hours—in 1878, and the whole of the theory of light was given in that time. Newton's spectrum was first explained and illustrated by coloured diagrams and the story was then told of Stokes's anticipation of Kirchhoff's discovery of the method of spectrum analysis. The phenomena of fluorescence and phosphorescence were explained, Stokes's theory being given, and Becquerel's recent experiments were described. The practical application of phosphorescent material to clock faces was then shown. The students were invited to come down and see for themselves the coloured images formed by polarized light passing through glass under compression, quartz, &c. But even the most enthusiastic of us were beginning to get fagged before the professor gave any signs of concluding. That last lecture remains clear in our memories.

Some years afterwards, the author attended Stokes's lectures on the same subject. His calm reflective style was a great contrast to Thomson's impetuosity, and the primitive apparatus he used, although admirably adapted for its purpose, was very different from the elaborate apparatus Thomson generally employed. Both had singularly winning smiles when lecturing, both were actuated by the same enthusiasm for science, and the relations of both to their students were marked by the most perfect old-world courtesy. Stokes was a scholar and a scientific man, but Thomson was in addition a man of affairs.

In the natural philosophy class a thorough knowledge of Kepler's laws and Newton's deductions from them was regarded as essential. Thomson used to mention several astronomical treatises, but advised students to read Sir John Herschel's, not because it was the most accurate or contained the latest discoveries, but because of its literary charm. The first paragraph of the introduction especially excited his admiration.

" In entering upon any scientific pursuit, one of the student's first endeavours ought to be to prepare his mind for the reception of truth, by dismissing, or at least loosening his hold on, all such crude and hastily adopted notions respecting the objects and relations he is about to examine as may tend to embarrass or mislead him: and to strengthen himself, by something of an effort and a resolve, for the unprejudiced admission of any conclusion which shall appear to be supported by careful observation and logical argument, even should it prove of a nature adverse to notions he had previously formed for himself, or taken up, without examination, on the credit of others. Such an effort is,

in fact, a commencement of that intellectual discipline which forms one of the most important ends of all science. It is the 'euphrasy and rue' with which we must 'purge our sight' before we can receive and contemplate as they are the lineaments of truth and nature."

Sir John Herschel was another in the long list of distinguished senior wranglers. It is highly probable that Sir Isaac Newton was first in his degree examination, but no record of his place has survived. Thomson regarded a knowledge of Newton's *Principia* and Herschel's *Astronomy* as essential to a liberal education, and it is interesting to remember that his tomb in Westminster Abbey is very near those of the two men whose works he delighted to praise.

The young professor entered on his duties at the beginning of the University session in November, 1846. In those days the session lasted only six months, November to April. For the rest of the year professors and students were free to occupy themselves as they pleased. For many students this long vacation was indispensable, since they had to work during one half of the year to make as much money as would keep them during the other half.

Thomson's first duty, apart from his teaching, was to make arrangements about securing proper apparatus for the work of the class. Nowadays this would chiefly mean equipment for the instruction of students in the physical laboratory. But in those days there was no laboratory, and the apparatus required was for the purpose of actual demonstrations in the lectures. Many years afterwards (in 1885, at Bangor) Sir William referred to this point in the following terms: "When

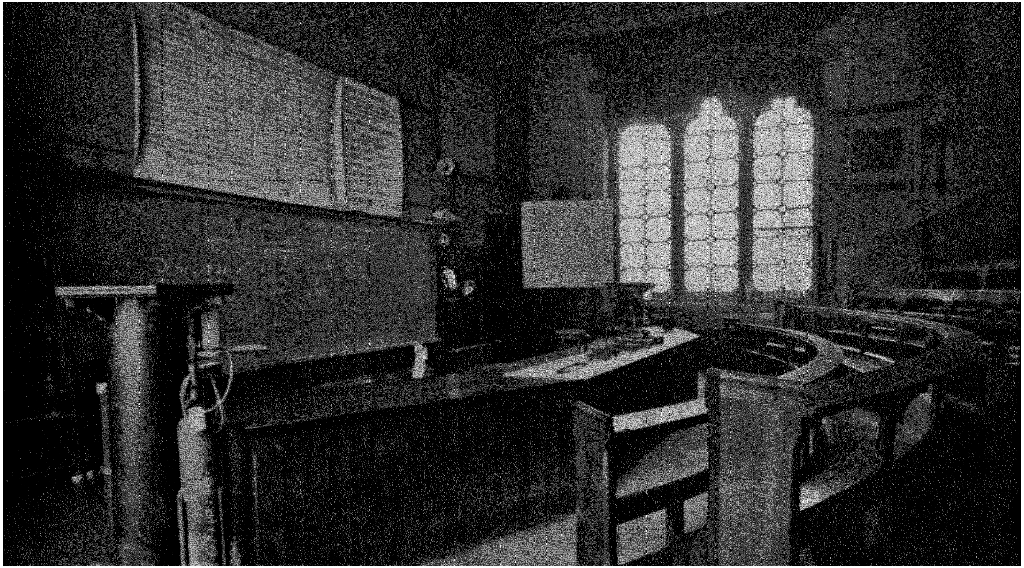
I entered upon the professorship of natural philosophy at Glasgow I found apparatus of a very old-fashioned kind. Much of it was more than a hundred years old, little of it less than fifty years old, and most of it was worm-eaten. Still, with such appliances, year after year, students of natural philosophy had been brought together and taught as well as possible. The principles of dynamics and electricity had been well illustrated and well taught, as well taught as lectures and so imperfect apparatus—but apparatus merely of the lecture-illustration kind—could teach. But there was absolutely no provision of any kind for experimental investigation, still less idea, even, for anything like students' practical work. Students' laboratories for physical science were not then thought of."

The Faculty was quite alive to the necessity of doing something drastic on the question of apparatus. Indeed, they had had the matter under consideration for some years, and only deferred taking active steps until a new professor should have been appointed.

The working equipment made rapid progress, and by 1863, and probably earlier, the University Calendar could announce: "The laboratory in connexion with the class is open daily from 9 a.m. to 4 p.m. for experimental exercises and investigations, under the direction of the professor and his official assistant."

The exact date of establishment, at the old college in the High Street, of what Lord Kelvin has claimed to be the first of all physical laboratories for students, is uncertain. Kelvin referred to it as having grown up between 1846 and 1856. The room first used as the laboratory was an old wine cellar!

The earliest of his laboratory workers were students



Lord Kelvin's Lecture Room in the University of Glasgow

whom Kelvin invited to come and help him in some of the important researches which he was then initiating into the "properties of matter", especially those connected with electricity. Most of these students were preparing to become clergymen. Few teachers took a University course at that time, but every student proposing to enter the ministry of the Church of Scotland, or some other of the principal denominations, and every man who wished to take an arts degree, had to go through the natural philosophy class.

Chapter IV

HOME LIFE

LORD KELVIN was twice married, his first wife being Margaret Crum of Thornliebank, daughter of Walter Crum, F.R.S., whose wife was a first cousin of Thomson's mother. The marriage took place on 15th September, 1852, and their short honeymoon was spent in Wales. At the end of the season they made an extended tour round the Mediterranean, visiting among other places Gibraltar and Sicily. In Sicily they travelled in any available conveyance, and often walked for long distances, putting up for the night wherever they could find any shelter. Miss King, Thomson's niece, tells us in her interesting book, *Kelvin the Man*, that the rough comfortless accommodation and the over-fatigue in travelling were too much for the physical strength of Mrs. Thomson, who was only twenty-two and whose indomitable spirit prevented her from giving up until she was utterly exhausted. This very probably led to the very long and very serious illness which made her for the rest of her short life a great invalid. The loving care taken of her by her young husband was most touching. He carried her up and down stairs, sitting with her and soothing her when the pain was severe and doing everything in his power to make her life more pleasant.

In December, 1860, a great misfortune fell on Professor Thomson, who when staying at Largs in winter was in the habit of indulging in his favourite game of

curling. The following extract from a letter from Mrs. Thomson to their friend Professor Helmholtz relates the disaster. It is addressed from Auchinean, Largs, and is dated 11th January, 1861. "We are detained by a most unfortunate accident which happened to Mr. Thomson three weeks ago to-morrow. He came down here on the 21st to spend the extra holidays with our friends Mr. and Mrs. Lang, I having come down a week sooner. On the 22nd he went to a frozen pond a mile or two distant to curl along with some friends. They were very late in returning, and at last Mr. Lang came to tell us my husband had fallen and hurt himself, but would soon be home. He was standing on a board, underneath was a narrow piece of wood, the board swung round with him and he was thrown with great violence on the ice. He attempted to rise but fell again immediately, and had to be carried home in great pain. It was necessary to give him chloroform in order to examine the limb, after doing which Dr. Kirkwood concluded there was fracture of the neck of the thigh-bone, but wished to have further advice. We sent to Glasgow for the professor of surgery, but he being in London, another physician came."

Two further doctors were consulted who were doubtful whether it was a fracture or not, but they decided to treat it as such. As Mr. and Mrs. Lang could hardly be subjected to the trouble of having an invalid for several weeks at least, another house (Mrs. Thomson's father's) was got ready and the invalid moved over to it on a litter. His leg was then set and put in splints. He suffered much from the effects of chloroform, and the pain caused by setting the limb was very great, as it was found to be an inch and a half shorter than the

other leg and had to be stretched to the same length. Convalescence was longer than anticipated, but in April Thomson was beginning to limp about on crutches. Mrs. Thomson wrote Helmholtz that the limb was quite stiff; he could move it a little up and down at the knee, but not one inch to right or left, and the injury left him permanently lame. Before very long Thomson was able to walk quite fast, limping along with his left hand pressed against his hip.

Next summer at his favourite Glen Cloy, Arran, he wistfully remarked that he would never again be able to climb Goatfell—and he never did, although his activity in after years was wonderful.

It was during that long illness, when forced for many weeks to lie on his back, that he began to use those large green-backed notebooks which always afterwards he carried about with him in a large special pocket. If he had to wait for a train or was otherwise delayed, he would take out this book, and put down suggestions for new experiments, calculations, designs for instruments, paragraphs for scientific papers, &c. The green book was always with him. Admiral Lord Fisher in his memoirs says:

" Lord Kelvin had a wonderful gift of being able to pursue abstruse investigations in the hubbub of a drawing-room full of visitors. He would produce a large green book out of a gamekeeper's pocket he had at the back of his coat and suddenly go ahead with figures." His green book was his inseparable companion. He left over a hundred of them; they show him as a tireless worker who utilized all his spare time to the best advantage, and they contained the germs of his subsequent inventions.

When Lady Thomson died in 1871, Thomson was heart-broken, and found it difficult to take up life with his former natural zest. He tried yachting; and it was at this time that he bought his sailing yacht, the *Lalla Rookh*, which opened up for him a new world of interest and pleasure and directed his mind into channels which led to his most valuable and best-known inventions.

He spent a large part of every summer vacation on board the *Lalla Rookh*, exploring the lovely west coast of Scotland, with its lochs and islands, Skye, Arran and many others, and he once sailed as far as Madeira, whence, in 1874, he brought home his second wife, Fanny, daughter of Charles Blandy of Funchal.

Thomson was an enthusiastic and perhaps slightly overdaring sailor, but as he had a good captain and crew, mishaps were almost non-existent, although sea-sickness often prevented his guests from enjoying the yacht as much as he did. Fortunately the second Lady Thomson was a good sailor.

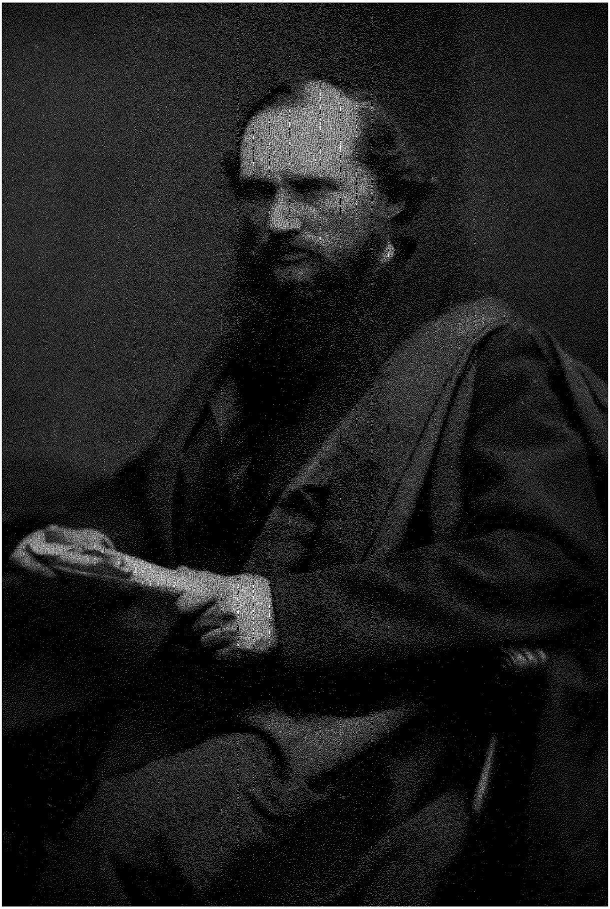
In 1870, Thomson was offered a chair at Cambridge University in connexion with the foundation of the Cavendish Laboratory, but his heart was in his work in Glasgow and he refused the offer. Glasgow had given him the freedom of the city in 1866, and he had enjoyed himself so much there, and was so free to work exactly as he wished, that he was quite resolved to spend the rest of his life in its neighbourhood. His academic duties had been immensely lightened by his official assistant Mr. McFarlane and by his demonstrator, deputy lecturer and nephew, James Bottomley, and other colleagues and helpers to whom we shall refer later on.

Thomson was always extremely practical, and his

expressed opinion was that the life and soul of science is its practical application. He had taken a large part, as we shall see later, in laying the Atlantic cable, which gained him his knighthood from Queen Victoria. In 1871 he designed a new mariner's compass, which is now almost universally used in ships and on which he made continual improvements up to the year of his death. He invented a deep-sea sounding machine which made navigation much safer, as it could be used when going at full speed, whereas the old rope method was possible only when the vessel was stopped. He patented a tide gauge which gave far more accurate results than any previous one. He made a water tap which never leaked. Out of all these and numerous other patents he made a very great deal of money, and as he had a good head for business, before he was fifty he was a rich man. He had never, indeed, been troubled with money matters since he was twenty-five. He was a partner in a firm which made scientific instruments and the actual cable used in deep-sea telegraphy. A second business, Thomson, Varley and Jenkin, brought him in a large income, while Thomson and Jenkin, as consulting engineers to various cable companies, earned several thousands a year each.

In England it was not till 1889 that the Admiralty decided to make Thomson's compass the standard for the Navy. But Germany had installed it long before we did.

The year 1874 was a year of great importance to Thomson. He had been sent out to Madeira as a scientific expert to discover and report on a fault in a cable, and was there some weeks, during which time he made friends with the Blandy family of Funchal. On leaving,



Sir William Thomson, 1870

his last good-byes were for them. A little later he returned in his *Lalla Rookh,* and proposed marriage to Fanny Blandy. They were married on 4th July, 1874, spent their honeymoon on the island and returned to Glasgow in the *Lalla Rookh.* The marriage was very happy. The second Lady Thomson, soon to be Lady Kelvin, was a very dignified and handsome woman, but she had a kindly and affectionate nature and made a most capable and willing hostess to her new nephews and nieces.

It was in 1874, too, that the Thomsons decided to build a large country house at Largs, and bought a piece of land on the banks of the Noddle. This house was largely designed by Thomson himself, who hired a master carpenter and other workmen to work to his plans. Perhaps this was not the cheapest or most successful way of obtaining a first-class residence, but the Thomsons were rich enough not to object to the twelve thousand pounds it cost, and they subsequently bought more land to enlarge the grounds. It was named Netherhall, and although not showy was a very comfortable Scottish mansion. Thomson was pleased with it, although the charms of the *Lalla Rookh* prevented him spending as much holiday time here as had been intended. Later on, in 1899, when he resigned the Glasgow chair, he retired here to live.

In the eighties Thomson became keenly interested in electricity, and in 1881 he decided to have electric lighting installed in his house in the University grounds, telling the workmen to put a light wherever there had been a gas bracket. This was successfully done, and it was the very first private house in Scotland to be lighted by electricity. Swan lamps were used, with a Clark

gas-engine and Faure cells. Two years later, he presented his old college, Peterhouse at Cambridge, with a complete installation of electric light, consisting of a small boiler and horizontal engine driving a Ferranti alternator, with a Siemens exciter. This installation was still doing excellent work twenty-five years later.

Years afterwards when the electric lighting of houses had become common, the professor then resident applied to be put on to the town mains. This was refused unless the house were entirely rewired, on account of the regulations of the electric supply not allowing naked wires, which Kelvin had always preferred—so time changes even the methods of great scientists.

During his long life Kelvin was the recipient of innumerable honours both from his own country and from many foreign lands. So numerous are they that one wonders if he remembered them all himself. His knighthood in November, 1866, was given him at Windsor by Queen Victoria, " in token of her appreciation of his services in connexion with the Atlantic telegraph ". In 1884 he became a knight of a Prussian order, and France made him a grand officer of the legion of honour in 1889. In 1890 Belgium made him a commander of the order of Leopold, and in 1892 he was raised to the peerage with the title of Baron Kelvin of Largs. This year also saw him made a freeman of the City of London.

In 1896 he was made a Knight Grand Cross of the Victorian order, and in 1901 Japan gave him the order of the first class of the Sacred Treasure. In 1902 he was given the much coveted Order of Merit and was made a Privy Councillor.

He was a member of numberless scientific societies both here and abroad, and received many gold medals. He served often as President of these societies, among them being the Royal Society of London, the Royal Society of Edinburgh, the Physical Society, and the Institution of Electrical Engineers. The Society of Telegraph Engineers was founded in 1871, and in 1874 Sir William Thomson was President. In 1882 the society became the Institution of Electrical Engineers and he was President in 1889 and in 1907, the year of his death. He was also three times President of the Royal Society of Edinburgh. He was given twenty-one honorary doctorates, chiefly in law, although Heidelberg preferred to make him a doctor of medicine, a source of great merriment in the family. Then he had doctorates from Dublin, Oxford, Cambridge, Edinburgh, Ticinensis, McGill University of Montreal, Columbia University, New York, Bologna, Padua, Manchester, Glasgow, Princeton, Budapest, Toronto University, Toronto College, Christiania, Wales, Yale, London and Leeds.

Although Lord Kelvin never had children of his own, his affection for his relations was very marked, particularly for his widowed sister, Mrs. King, at whose home in Hamilton Terrace, London, one room was always his, and was kept ready and vacant for him at any time without notice. He had a latchkey, and often came in to breakfast and surprised them. But on one occasion he mislaid his key and was quite unable to effect the burglarious entrance he attempted, and when at last rescued from the doorstep was in a very chilly condition. He never caught colds, and when remonstrated with for appearing on the deck of his yacht on a very cold day without his overcoat, said he was never

cold, as wherever there was fear of it he put on another thin vest or waistcoat, thus copying the Chinese, who in many ways are ahead of us. He was exceedingly fond of his nephews and nieces, one of whom, Miss King, tells how " loving and lovable " he was. Another niece married Dr. Gladstone, F.R.S., of London, but died at the birth of their only child, a daughter. This daughter, Margaret, used to stay at Netherhall with her great uncle and aunt, and was amused by the difference in their manner of entertaining their friends. Both were amiable and hospitable, but whereas her aunt loved filling the house with company and looked after them well, her uncle, in spite of his very friendly feelings towards them, would often go into a brown-study and scribble long calculations in the current green book, entirely ignoring his surroundings. Margaret loved best those days in which she had them to herself. A photograph of her at this time shows us a charmingly pretty girl of sixteen with a sweet and softly feminine expression, and dressed in a simple evening gown with a bead necklace. Not much later the Kelvins were surprised and a little shocked to hear that she had married a Mr. Ramsay Macdonald who worked in London. This was the future Prime Minister. Margaret gave him five children and a most happy though all too brief married life; to the lasting sorrow of her husband she died while still young. Lord Kelvin saw a good deal of Ramsay Macdonald at Mrs. King's house and had a high opinion of him.

Chapter V

SUBMARINE TELEGRAPHY

FROM 1840 to 1850 great improvements had been made in land telegraphy, and popular imagination was stirred by the mystery of the electric method of signalling. Practically every leading newspaper had a paragraph or column headed " By Electric Telegraph ". People had begun to realize that the combination of science and engineering might lead at any moment to still greater marvels. The possibility of connecting the New World with the Old by means of an electric cable was warmly discussed, but its feasibility as a commercial venture was denied by many competent engineers.

It was a notable day when early in 1850 the Gutta Percha Company received what they regarded as a very large order, for twenty-five knots of copper wire, No. 14 Birmingham wire gauge, a knot representing 2029 yards. It was required for the purpose of laying a line across the Channel from Dover to France and so attempting to prove the practicability of submarine telegraphy. It was thought at the time to be a very daring experiment. The wire drawers of these days were ignorant of the proper methods of annealing wire and never attained high accuracy in the measurement of its diameter. No measurements whatever were made of the electrical conductivity of the wire, which as a natural consequence varied considerably, even when the wire was supplied by one firm. The Gutta Percha

Company could cover only short lengths of the wire with insulation, and the thickness of the insulating material was far from uniform. To suit the requirements of the covering machine, the lengths of wire had to be joined by overlapping scarves which were soldered together, the joint being then filed to the size of the wire. When the wire was covered before the soldering was done, this process did not answer properly, as the wire became so hot that it softened the gutta percha for an appreciable distance on each side of the joint. A new method had to be devised, and so the gutta percha was removed for two inches at each end of the wire, and these ends, after being well cleaned with emery paper, were crossed and twisted together one over the other; the whole was then covered with solder applied by a soldering iron, powdered resin being used as a flux. A slice of plastic gutta percha was then placed on each side of the joint and the whole pressed together in a wooden mould until the gutta percha was quite hard. When removed from the mould the joint looked like a magnified cigar, some two inches in diameter and nine in length; it tapered at each end to the size of the covered wire over which it lapped. The electrical arrangements were very simple, and the battery, divided by wooden partitions into twelve cells, was unsuitable for the work it had to do. The galvanometer used was of the vertical needle type and was far from sensitive.

Eventually the coils were carted to a wharf on the Thames and shipped on to a small steam-tug called the *Goliath*. Just behind the funnel of this tug a large iron reel was mounted on suitable bearings, its ends nearly reaching the bulwarks on each side. Upon this reel the twenty-five knots of covered wire had to be wound

as evenly as possible. During the process of winding and testing the cable, crowds assembled on the quay. One observer is stated to have exclaimed: "What a mad scheme! Why, any sailor would tell you it was impossible to pull such a line twenty-five yards, over such a rough and uneven surface as the bottom of the Channel." He evidently thought that the signals were to be made by pulling at the wire after the manner of mechanical house bells! Another bystander took a more optimistic and sensible view, remarking: "Why, when they said that the railway was coming to Dover through Shakespeare's Cliff, there were many knowing shakes of the head and laughs of derision; but it was accomplished for all that, and these men will be equally successful."

On 23rd August, 1850, at 10 a.m., the weather being very propitious, the laying was begun from the *Goliath*, which finally anchored in the vicinity of the shore end buoy off Cape Grisnez. The sea end of the line was then passed into the cabin of the tug, with a type printing instrument. But something seemed to have gone wrong with the operator at Dover, and although letters came through they were so mixed up that it was impossible to make sense out of them. The more the operator tried to control the letters the more erratic they became. The steamer then left, and the end of the line was passed into a boat, where it was joined to the shore end of the line in the lighthouse. But although messages appeared to be sent correctly, no other message came; and when letters arrived from Dover, they learned that their signals had been received there in the same chaotic order. The electricians tried every device they could think of to make the cable work, but without success.

Accustomed only to the clear sharp signals of land lines, they could make nothing of those got from the cable. This was the earliest experience of the effects of electrostatic induction in retarding electric signals and altering their character.

It was in 1854 that Thomson's attention was drawn to the subject by Stokes, and in this way began his connexion with submarine telegraphy, which was to have such important effects on its development. Thomson was then a young man of thirty, but had already for eight years been a professor at Glasgow; little known to the general public, but with a European reputation among scientific men. Thomson attacked the problem of submarine telegraphy immediately his attention had been drawn to it, and in less than a fortnight he sent a solution to Stokes. The complete solution he published in the *Proceedings of the Royal Society* for May, 1855. In this paper he points out that the effect of electrostatic induction is to make the flow of electricity analogous to the flow of heat from a hot to a cold part of a solid body. He shows that the "speed of arrival" of a signal varies as the product of the electrostatic capacity of the cable and its resistance. It also follows that the time taken by the current to reach any particular fraction of the full value will vary as the square of the length. This result was of great importance, as a project for connecting England and America by wire was then being mooted. From experience already gained with short cables the law of squares seemed at first to give little prospect of ever telegraphing 2000 miles. Mr. Whitehouse, who was interested in the Atlantic project, challenged the accuracy of Thomson's conclusions. Controversial letters, published in the

Athenæum (October and November, 1856), ensued and Thomson showed that Whitehouse's experiments only confirmed the law.

These letters led to Thomson's taking a share in the earliest attempt to lay an Atlantic cable, in 1857. The Atlantic Telegraph Company had been formed in October, 1856, and in December Thomson joined the Board as a director, being elected to represent the interests of the Scottish shareholders. Charles Bright was engineer-in-chief, and Whitehouse was electrician. Thomson had no technical position beyond being a director.

Writing to Helmholtz in December, 1856, Thomson says: "The Atlantic telegraph is now in process of manufacture. Two thousand five hundred miles of cable are to be finished and ready to go to sea by the end of May, and if no accident happens electric messages will be passing between Ireland and Newfoundland before July."

In some respects the circumstances were favourable. For example, in laying cables in the Mediterranean, Alpine precipices and mountains had been found at the bottom of the sea. Between Ireland and Newfoundland there was a soft level bottom consisting mainly of fine sand and microscopic shells, and nowhere was the depth more than 3½ miles. The cable, too, was much lighter than any hitherto laid. It weighed only 18 cwt. per mile in air, and in water only 8 cwt. per mile.

To lay the cable it was coiled on board two ships of war, the British battleship *Agamemnon* and the United States frigate *Niagara*. On 5th August, 1857, the shore end was landed at Valencia, Kerry, Ireland, and the *Niagara* began to pay out. The plan was for the *Niagara*

to pay out her section first, and for the *Agamemnon* to continue the work after splicing the ends of the two sections in mid-ocean. But the paying-out gear was very crude, and the brake for maintaining the cable at the proper tension difficult to regulate. After 300 miles had been laid, there was a mishap at the brake, and the cable parted at 2000 fathoms (12,000 feet). The ships were obliged to return to Devonport, new machinery was designed, and 700 miles of fresh cable manufactured in readiness for the attempt to be made the following year.

Thomson had joined the expedition at the request of his brother directors and was on board the *Agamemnon*. He came back full of ideas on both the electrical and mechanical sides of the problem.

In the early summer of 1858 the ships were again ready to put to sea. Thomson had succeeded in establishing systematic tests of the conductivity of copper. He had also invented the mirror galvanometer which was destined to make Atlantic telegraphy feasible. This time the two ships, after encountering a great storm which damaged the coiled cable severely, met in mid-ocean, spliced the cable and paid out simultaneously, the *Agamemnon* steaming towards Ireland and the *Niagara* towards Newfoundland. After six miles had been paid out the cable broke. Again the ships met and a fresh splice was made, and again the cable failed when about eighty miles had run out. A third attempt was made, only to lead to another breakage. The ships returned to Queenstown, with the leaders of the expedition disappointed but not disheartened, and their advice to the Board to order a fresh attempt was accepted. The ships once more met in mid-ocean, and this time

success crowned their efforts. On 5th August each ship completed its task, the ends of the cable being brought to land, and congratulations were exchanged between Great Britain and the United States. Unfortunately, however, the working of the cable soon ceased to give satisfaction. Mr. Whitehouse, who was engineer at the Irish end, attempted with little success to establish communication by means of his own signalling instruments. When the Thomson galvanometer was used, however, with a simple Daniell battery, messages were successfully sent. The Board replaced Whitehouse by Thomson, and some important messages were transmitted; but again on 6th September signals ceased to pass. In the report of the failure of the cable, it was stated that it was not well made and that the strains made on laying it had been too great and unequal. It was found impossible to repair it and it was abandoned.

During its short life 732 messages had been sent, some of which were of great importance. For example, by its means the orders for two regiments of English soldiers to leave Canada in order to assist in quelling the Indian Mutiny were countermanded. This is estimated to have saved the country at least £50,000. So it will be seen that this cable was of more than merely theoretical value.

It was not until 1865 that a third attempt was made to lay an Atlantic cable, but important work had been done in the interval. A committee of the British Association had, at Thomson's suggestion, begun to design standards for electrical measurements. Cable engineers also had been gaining valuable experience. Improvements had been made in the paying-out machinery. A type of cable was devised which was much more

suitable for bearing the stress of laying, and especially the much severer stress of picking up. Thomson encouraged a fresh attempt. " What has been done," he said, " will be done again. The loss of a position gained is an event unknown in the history of man's struggle with inanimate nature." It required some courage to engage in the enterprise, for at this time it was said that out of 9000 miles of cable already laid there were only 3000 in working order.

In 1851 the Great Exhibition in Hyde Park displayed to an astonished world the wonders of science and engineering. The business world was alive with optimistic and grandiose ideas, for in the last twenty years England had been covered by networks of railways and telegraphs. Steamships were finding their way into every ocean, and as there appeared to be no limit to expansion, a huge ship, named the *Great Eastern*, had been built. No other ship in 1865 could have taken on board the 3000 miles of cable needed for connecting Valencia and Newfoundland.

The *Great Eastern* was originally built with the object of steaming to Ceylon and back without recoaling. After great difficulty she was launched on 31st January, 1858. She was at least five times larger than any previous vessel, and was therefore a daring experiment. Her principal dimensions were:

Length, 692 feet; breadth, 82⅛ feet; depth, 58 feet. Power of screw engines, 4886 h.p.; power of paddle engines, 3411 h.p. Coal capacity, 10,000 tons. Displacement, 27,384 tons. Speed, 14·5 knots.

Captain Edgar G. Smith says that apart from commercial considerations this premier leviathan stands out as a wonder and pattern of naval construction.

Had the machinery equipment been more advanced at this period she would have been a commercial success, but from this point of view she was a failure and her useful career closed in 1873. During her life she laid three Atlantic cables, the French cable to America, and the cable from Suez to Bombay.

In 1865 the *Great Eastern* was chartered to lay the cable. In this expedition Thomson, accompanied by Cromwell Varley, went as a consulting expert on behalf of the company. Twelve hundred miles were successfully laid and then a fault developed. Picking up was begun, but in manœuvring the ship the cable parted in deep water. Attempts were made to recover it by grappling. Three times it was hooked and brought part of the way to the surface, but the shackles used to couple up successive lengths of the grappling-rope were too weak to stand the strain, and grapnel, rope and cable were lost. The ship returned with the task unfinished, but every one on board believed that not only would a sound cable be laid but that the lost cable would be recovered.

In 1866 an entirely new cable was laid with complete success. The *Great Eastern* with her consorts proceeded to the lost end of the 1865 cable and began once more the difficult task of fishing for it in water 12,000 feet deep. A fortnight passed and then the watchers at Valencia saw the spot begin to flicker and the signals when written as letters formed words. A few days more and this cable too worked satisfactorily.

During these operations Thomson was in the ship, Varley remaining at Valencia. Thanks to their labours, and to those of Mr. Willoughby Smith, the contractor's electrical engineer, the appliances used for testing on

board ship had been developed most satisfactorily. It was now generally recognized that most of the credit for Atlantic telegraphy belonged to Thomson, although characteristically he disclaimed having played the leading part. Thomson, along with Mr. Canning, the engineer of the expedition, and Captain Anderson, who commanded the *Great Eastern*, received the honour of knighthood.

For a time Thomson's mirror galvanometer was the only instrument by which conversations were possible. He then designed an instrument which gave a record of the electric impulses and thus saved the need for having a skilled attendant to take them down. He secured greater available mechanical power in the device by interchanging the function of the magnet and coil, the latter being now made very light and the magnet, which was stationary, very heavy. The coil was placed in an intense magnetic field between the poles of the magnet and the movement of the coil actuated a very light pointer in the shape of a very fine-drawn siphon-shaped glass tube. From this tube ink was deposited on a running paper band. This was the earliest type of a moving-coil galvanometer, and was the forerunner of the d'Arsonval and other types of moving-coil instruments. It was essential that Thomson's type of instrument should move without friction, and he effected this very successfully by electrifying the ink so that ink and paper attracted each other. The result was that the siphon was maintained in a constant state of vibration, alternately advancing to the paper, where it deposited a minute drop of ink, and then springing back, but all the time free to follow without friction the movements of the coil in obedience

to the electric impulses arriving through the cable. Dynamically the siphon-recorder satisfies the same conditions as those that determined the design of the mirror galvanometer. It draws on the moving strip of paper a curve of arrival for every one of the successive currents of which the signals are composed.

To this day some form of Thomson's recorder remains in almost universal use as the standard instrument in submarine telegraphy. It has been simplified by the substitution of permanent field magnets for electro-magnets and by the use of an electromagnetic vibrator for the siphon instead of electrification. These changes were made in later years by Thomson himself.

In 1865 Thomson formed a partnership for telegraphic patents with Cromwell Varley and Fleeming Jenkin. Varley's chief contribution was the highly important device of signalling through condensers, and Jenkin was associated with Thomson in the invention of what is known as "curb sending". This invention makes the signals through a cable more definite by following up each signal current with a reversed current of shorter duration, instead of putting the cable to earth. The effect is to get a quicker emptying of the cable in preparation for the next charge. Much ingenuity was expended in perfecting apparatus for carrying out this principle, but its use was not extensive and practically the same result is now obtained by other means.

Besides this triple partnership in patents there was a separate partnership between Thomson and Jenkin which lasted until Jenkin's death, under which they acted as consulting engineers for the construction and laying of submarine cables.

Among the cables for which they acted was the

Western and Brazilian line, the first section of which was laid in 1873 by the steamship *Hooper*. Sir William Thomson went out in the *Hooper* on her first trip, and at Madeira, where there was some delay through the cable having to be turned over in the tanks to cut out a fault, he met the lady who afterwards became his second wife. The Blandy family, who were the principal residents in Madeira, were very hospitable to Sir William. In a letter he wrote to his sister, Mrs. King, he says: "We had some admirable lamp signalling several evenings at Funchal between the *Hooper* and Mr. Blandy's house, about 1½ miles away. The Miss Blandys learned 'Morse' very easily and quickly and both sent and read long telegrams the first evening they tried it. When the cable was all packed and the *Hooper* sailed away, a figure was seen at a high window in the Blandy's house, with a white scarf waving a message in Morse. 'Eh! What's that? What's that?' said Sir William, adjusting his eyeglass to read the signal. It was 'Good-bye, good-bye, Sir William!'" That good-bye brought him back again the following May in the *Lalla Rookh* to ask Fanny, Mr. Blandy's second daughter, to marry him.

At this period the test room of a cable factory was a better school in which to study electricity than any lecture room or laboratory, for the electricity of the practical engineer had outstripped that taught in the schools. The telegraph engineer of the sixties and seventies used methods of exact thinking which laid the foundation of the electrical engineering of to-day. Thomson and Fleeming Jenkin were closely connected in telegraph work, with patents and with the determination of the electrical units.

The following reminiscences by Mrs. Fleeming Jenkin which Sir Alfred Ewing read to the Institution of Electrical Engineers in his Kelvin lecture, delivered in 1910, give a lively picture of Thomson.

"My recollections date from the autumn of 1859, when I first saw him. Fleeming had told me much about him, speaking of him with great affection, but also with awe-struck admiration and veneration, so that I pictured to myself Professor Thomson as an aged and severe philosopher and rather dreaded an introduction to him. One evening I was sitting reading by lamplight, when I heard hurried footsteps coming up the stairs; the door opened and in came a tall fair-haired young man, who, not waiting to be announced, and with a most radiant smile, said: 'Where is Fleeming? Are you his wife? I must see him. I am William Thomson.' Then he spoke a few kind words of congratulation on my recent marriage, and I saw for the first time that benevolent bending of eyes on the person to whom he spoke that always remained and increased, I think, with the years. But the splendid buoyancy and radiance—which made me say to my husband when he came in later, 'I have had a visit from Professor Apollo'—I never saw again. It was in the following winter that Professor Thomson met with the accident which lamed him for life.

"From that time forward I often saw Professor Thomson. He and Fleeming were experimenting together on submarine telegraphic instruments. As Thomson lived in Glasgow, our house in London became the place where the experiments were carried on, and our dining-room the workshop. Gradually, as the number of instruments increased, our dining space

became smaller, and in memory I seem to see Professor Thomson and Fleeming and me dining together hurriedly on a little island in the middle of the room, surrounded by an ever-rising tide of galvanometers, coils of wire and mechanism of all sorts. After dinner they used to set to work, and work for hours. I used to make coffee and tea and carry it to them from time to time, and sometimes I was trusted to sit with a watch in my hand and count the seconds between one flitting flash of light and the next on the instrument then in hand.

"I say we dined hurriedly, because Lord Kelvin always did or seemed to me always to do everything at topmost speed. When he came, it was always in a hansom-cab, in front of which he stood urging the driver on and guiding him by his pointing stick to our house, the address of which he never could learn though he came constantly, and when he was whirled away just in time to catch some mysterious train which started for Glasgow at the earliest possible hour in the morning.

"He loved music and would listen to it in a sort of trance of enjoyment. But at the time he would admit none but German music to be music, and used vehemently to attack Italian music, which we admired. It happened, a day or two after such a discussion, that we had an opera box sent us. The piece was Rossini's 'Semiramide'. Professor Thomson arrived to dine and work as usual, but we carried him off to the opera, but did not say what opera was to be given. We were a few minutes late, and as we took our seats Trebelli began her great song. He listened in rapture and at the end said, 'Beautiful!' I held the play-bill out to him, pointing to the name of the composer, Rossini!

'Ah!' said he, 'but it *was* beautiful and I was wrong.'

"One more recollection is of a luncheon party at our house, to which he came; a very learned luncheon party; there were three senior wranglers present at it. The talk turned not unnaturally on scientific matters; Sir William, to illustrate what he said, and to prove that if a tumbler full of water were turned upside down in a certain way the water would not come out, took a tumbler, filled it, turned it upside down—and all the water poured down on to the table. Murmuring 'some error', he filled it again, turned it upside down again, and down came the water again, so that the table was all aswim. Terribly sorry and begging pardon in his kindest way, he became flurried, and dropped the tumbler, which broke in pieces. He was inconsolable and insisted on driving round by a shop on his way to the station to buy me a tumbler in place of the broken one. In the greatest hurry, and in spite of all we could say, he caught up the broken pieces to match them and drove off. Soon he came back in triumph, waving a tumbler from the cab window. Fleeming ran out to receive it: Sir William drove off: Fleeming brought in the tumbler: it did not match!

"And then the last time I saw him, not many months before his death, when his kindness and courtesy were, if possible, more beautiful than ever; when he insisted on giving me his arm and bringing me out to the carriage I was in, though he was so weak and worn with pain and age."

Since the first cable was laid the technique of submarine telegraphy has vastly improved and its use has extended enormously. At first the speed of working was about eight words per minute, but subsequently this

was doubled by using condensers at each end. At the start the tariff was £20 for twenty words and £1 for each additional word. People have long ceased to regard it as wonderful. A message is handed in at a telegraph office and a few minutes afterwards it is received by the person to whom it is sent, in some city perhaps thousands of miles away. The submarine telegraph is regarded as the handmaid of industry, and few people think of what has happened to the message during the few minutes that it takes for the journey. The vastness of the overseas telegraph communications of to-day is shown by the number of cables. There are now (1937) about 1500 submarine cables, measuring nearly 370,000 nautical miles and linking up every part of the civilized world. Of this number about 245 cables, representing nearly half the world's total mileage, are owned and operated by British capital and enterprise. The length of these is sufficient to encircle the earth over seven times.

The efficiency of the telegraph cable has also increased tremendously in recent years. So far as communication is concerned, none of the world's cities is more than a few seconds away from London. The London Central Telegraph Office has been called the pulse of the world, as messages are continually coming in from all quarters of the globe and are being dispatched to every point of the compass.

Direct cable routes in which instantaneous automatic retransmission only is used are provided from London to Cape Town, Egypt, Bombay, Colombo, Singapore, Hong Kong, Australia, New York, Montreal, Rio de Janeiro and Buenos Aires. This greatly speeds up traffic on the main routes and makes the exchange

of urgent messages with almost any other station in the world a matter of minutes.

A stockbroker in London can now hand in a message for New York and have a reply in twenty-four seconds. A signal from London to Australia can, without the intervention of any human agency on the way, be transmitted in less than two seconds. This is made possible by the use of regenerator and relay stations in the circuit. This system not only amplifies the signals in the process of transmission but corrects their distortion due to the electric properties of the circuits.

A master clock in London keeps the regenerator equipment in synchronism and so messages are automatically transmitted through long chains of cables, the apparatus at the receiving stations responding directly to the automatic transmitter in London. This system also permits transmission through two or more channels in one cable, and this enables two slower cables to be linked up with one faster section, which then carries the traffic of both. On the London to Hong Kong chain alone there are thirteen intermediate relay stations.

Since the *Great Eastern* sailed the Atlantic in the sixties of the nineteenth century much has been accomplished. Thomson's mirror galvanometer, which had a maximum speed of about ten words per minute, was replaced by his own "recorder", and this in turn by the Muirhead automatic transmitter of the nineties. At the beginning of the present century the automatic cable relay came into use, and about twenty years later the system of duplexing, which permits simultaneous operation in each direction.

Then came the tremendous improvements in tele-

phone transmission by means of loading coils and methods suggested by Heaviside and Pupin, which prevented distortion of the signals and made much cheaper cables available. The discovery of "permalloy" by G. W. Elmen, in the Bell Telephone Laboratories, a material which has novel magnetic properties peculiarly suitable to the requirements of telephony, has made long-distance telephony through submarine cables a rival to submarine telegraphy. In addition there is also radiotelephony, in which very rapid development is continually taking place.

From all this it will be seen how great is the debt we owe to the early inventors and particularly to Thomson for laying the foundation on which this gigantic superstructure of practice and theory has been built.

CHAPTER VI

THOMSON AND TAIT'S *NATURAL PHILOSOPHY*

IN 1860 P. G. Tait was elected to the chair of natural philosophy in Edinburgh University. He had been senior wrangler seven years after Thomson took his degree, and had been educated at the same Cambridge College, Peterhouse, and under the same coach, Hopkins. There were, therefore, many points of similarity between his career and Thomson's. Tait was the first to plan the great literary undertaking of writing a complete treatise on natural philosophy, and he was surprised and delighted when Thomson suggested that they should write the treatise jointly. They started with the intention of discussing in succession the various branches of natural philosophy, both experimentally and mathematically.

In 1867 the Clarendon Press published the first volume of the series. It was mainly introductory, and gave a very complete account of the science of force (dynamics) and its action in maintaining rest or producing motion. The treatment of the subject is that given by Newton in his *Principia*, and the book marks the beginning of a new epoch in dynamical science. The high standard the authors set themselves made it necessary for them to solve many difficult and abstruse problems, and owing to the large encroachments this

made on their time, they soon saw that it would be impossible for them to complete their great undertaking.

In 1872 Thomson published his *Reprint of Papers on Electrostatics and Magnetism*, and in 1879 the Cambridge University Press published the first part of a new edition of the first volume of the *Natural Philosophy*, revised and greatly enlarged. In 1883 the second part of this volume was published under the editorship of G. H. Darwin. In the preface the authors state that they have definitely abandoned their intention of writing a complete treatise. They have, however, left in the references to future volumes, as their method of treatment can only be fully justified by taking into account the original design of the work.

The first sentence of Fourier's *Theory of Heat* is printed in the original French, at the head of their preface:

"Primary causes are unknown to us: but they are subject to simple and constant laws, which may be discovered by observation, and the study of which is the object of natural philosophy."

The first two paragraphs in their preface explain very clearly the aim they had in view:

"The term Natural Philosophy was used by Newton, and is still used in British Universities, to denote the investigation of laws in the material world and the deduction of results not directly observed. Observation, classification and description of phenomena necessarily precede Natural Philosophy in every department of natural science. The earlier stage is, in some branches, commonly called Natural History; and it might with equal propriety be so called in all others.

"Our object is twofold: to give a tolerably complete

Lord Kelvin's Library

account of what is now known of Natural Philosophy, in language adapted to the non-mathematical reader; and to furnish to those who have the privilege which high mathematical attainments confer, a connected outline of the analytical processes by which the greater part of that knowledge has been extended into regions as yet unexplored by experiment."

Before the publication of the treatise many believed that the work done against friction was absolutely lost. Newton, for example, believed this. Thomson and Tait, taking as a physical axiom that "the Perpetual Motion is impossible", prove the great principle of the conservation of energy. Energy like matter is indestructible. They show that every motion which takes place in Nature meets one or other of the following forms of resistance—sliding friction, viscosity or imperfect elasticity, resistance due to induced electric currents, and resistance due to varying magnetization. Our everyday experience proves that bodies falling freely in the air are impeded by its viscosity, and that friction greatly hampers the action of all kinds of mechanism.

The analogies of Nature lead us to believe that every star and every body of any kind has its relative motion hindered by the medium in which it moves. A curious result is that this frictional resistance tends to shorten the year, for it makes the earth move closer to the sun, and so the attraction between them is increased and the year thus shortened. Astronomical data prove that this shortening is appreciable, although very minute. A fraction of the year, therefore, cannot be used as a permanent unit of time. Neither is the time of rotation of the earth round its axis a constant. Newton explained clearly the action of the sun and the moon in producing

tides. Kant was the first to point out that the frictional resistance to the motion of the tidal waters in oceans, lakes and rivers acts as a brake on the earth's motion of rotation. It thus tends to equalize the lunar month, which is the period of the moon's rotation round the earth, with the sidereal day, or period of the earth's rotation round its axis. The authors speculate as to what will happen in the future, taking into account the effect of the solar tides. They conclude that the moon is moving farther from the earth and that the lunar month is lengthening. After attaining a maximum distance from the earth, it will gradually return to it in a spiral path, the month shortening, and it will finally fall on to the earth. G. H. Darwin has carried this investigation much farther, making use of it even to calculate the age of the earth. Starting with a planet in a partly solid and partly fluid condition and rotating round its axis in from two to four hours, he shows that the motion is unstable. It is therefore highly probable that the planet split into two, the larger portion being the earth and the smaller the moon. He then traces the relative motion of the earth and moon up to the present day, and computes that the minimum time required for the moon to have acquired its present distance is fifty-four million years.

The data for making these calculations are somewhat uncertain, but there can be no doubt as to the ultimate result. All the bodies of the solar system must ultimately fall together into one mass, which, although rotating for a time, must finally come to rest relatively to the surrounding medium.

Since neither the period of the earth's rotation round the sun nor that round its own axis can be considered

constant, the authors consider the question of the best standard of accurate chronometry. Their first suggestion was to make a metallic spring and hermetically seal it in an exhausted glass vessel. In their second edition they mention Clerk Maxwell's suggestion that the period of vibration of an atom of hydrogen or sodium would be an excellent natural standard. These atoms are all ready made: there is an infinite number of them all exactly alike in every physical property, and the time of vibration of an atom as determined by spectrum analysis is absolutely independent of its position in the universe. Clerk Maxwell also suggested to the authors that the time of revolution of an infinitesimal satellite, close to the surface of a globe of water, could be advantageously taken as the unit of time, as it is quite independent of the size of the globe.

Thomson and Tait lay great stress on the importance of carrying out experiments. Without these, terrestrial magnetism would never have been discovered. The same is true of the connexion between a lightning flash and the phenomena exhibited by rubbed amber.

They give rules for conducting experiments and, judging from their own very extensive experience, say that endless patience and perseverance in designing and trying different methods for investigation are necessary for the advancement of science. The experimenter who is likely to succeed is the one who does not allow himself to be disheartened by failure, but immediately sets about varying his method so as to interrogate Nature in every conceivable way.

They follow Herschel in pointing out the importance of residual phenomena. If, after making allowance for all known causes of error, there still remains an appre-

ciable discrepancy, it is of the greatest importance to investigate the reason for this. Adams and Le Verrier were led to discover a new planet by noticing very slight anomalies in the motion of Uranus. Schönbein was led to discover ozone—a gas of great chemical activity—by noticing that a frictional electrical machine produced a distinct smell when working.

On the other hand, when the agreement between our results is closer than we have a right to expect, it is probable that our apparatus is not trustworthy. For example, a very good achromatic telescope makes every star appear to have a sensible disc; but it would be rash to draw any conclusion as to the size of the star from this. Further investigation proves that it is merely a phenomenon due to the diffraction of light.

Until we know thoroughly the nature of matter and the forces which produce its motion, it is impossible to get rigorously exact solutions of physical problems. In order to obtain any solution, it is often necessary to make the problem easier by introducing limitations into it. The authors, as an example, take the case of a crow-bar used to move a heavy mass. If we introduce the limitation that the bar is perfectly rigid, it is easy to get a solution. Let us next suppose that it bends slightly, and with little greater difficulty we obtain a more exact solution. Then, supposing that the mass is perfectly homogeneous—which, owing to its atomic constitution, it never can be—and that the forces consequent on dilatation, compression and distortion are in exact proportion to these deformations, we can get a still more accurate solution. The complete discussion would involve the consideration of the deformations which take place in every part of the bar, fulcrum and mass,

and we should also have to take into account the heat generated and its conduction throughout the bar. Our ignorance of the nature of matter makes any such discussion impossible. But in many cases solutions of great value for practical work can be obtained by limiting the generality of the problem in the ways shown by experiment to be permissible.

It is interesting to notice that the proof which the authors give of the theorem that a sphere attracts an external particle as if all its mass were collected at its centre is purely geometrical and is practically identical with that given by Newton. Several terrestrial applications of Newton's law of gravitation are given. The attraction of a hemisphere on a particle at its edge is found and the result is used to compute the deflection produced by a hemispherical hill on a plumb-line at its base. Similarly near the edge of a hemispherical cavity an opposite effect would be produced on the plumb-line.

The more important theorems discovered by Thomson in the theory of elastic solids are incorporated in the second part of the treatise. Many of these problems are of very great difficulty, and the results, arrived at by very powerful and neat analytical methods, are of great importance. This section of the work is an excellent introduction to advanced problems in the theory of the subject.

One of the problems solved is to find the deformation of the solid earth, supposed to be a homogeneous sphere, by the tides produced by the moon and sun. Thomson was the first to point out that in any complete theory of the tides this deformation must be taken into account. All previous dynamical investigations of tidal pheno-

mena, and of precession and nutation, proceeded on the assumption that the outer surface of the solid earth is absolutely unyielding. In Thomson's opinion the mere fact of the existence of tides disproves the assumption formerly commonly made that we live on a mere thin shell of solid substance enclosing a fluid mass of melted rocks and metals.

If the earth were truly a thin shell covered with a thin layer of lighter liquid, the liquid would have practically the same depth all round. Under tidal influences it would simply rise and fall with the shell, and so the tides would be infinitesimal, land and sea rising and falling together. He calculates that a solid steel sphere of the size of the earth would yield one-third as much as a perfectly fluid globe, and that this yielding would reduce the height of the tides to practically two-thirds of what it would be if the rigidity were infinite. Sir George Darwin pursued this investigation farther and secured some interesting results.

When the liquid surrounding the globe has a greater density than the globe itself, unexpected results follow. The equilibrium in this case is unstable, but the complete investigation still baffles the skill of mathematicians.

Thomson considered the augmentation of the tides due to the mutual gravitation of the parts of the water itself; the results show that it is appreciable. Robinson had previously pointed out that the great tides in the Bay of Fundy would produce a very sensible deflection of a plumb-line in the neighbourhood. As this is due indirectly to the attraction of the moon, it is not correct to say that the moon has no appreciable effect on the deflection of a plummet. Even ordinary tides must produce at places near the sea an effect on the plummet

considerably transcending the direct effect of the moon. The suggestion is made that observation of this effect might be used to determine the earth's mean density.

In order to show Thomson's keen insight into physical phenomena, his discovery of a thermodynamic acceleration of the earth's rotation may be mentioned. It is well known that the barometer indicates variations of pressure during the day and night. The semi-diurnal constituent has its maximum values about 10 a.m. and 10 p.m. respectively. The crest of the nearer tidal protuberance is thus directed to a point of the heavens westward of the sun, and the solar attraction on these protuberances causes a couple about the earth's axis, which accelerates its rotation. As the barometric oscillations are due to solar radiation, it follows that the earth and the sun form a kind of heat engine. Thomson calculates that the earth gains about 2·7 seconds per century on a perfect clock. On the one hand we have this effect and the shrinkage of the earth tending to make the earth rotate more quickly, and on the other we have the tides and the fall of meteoric dust tending to make it rotate more slowly.

Looking back on the work done by Thomson and Tait—who used to refer to their treatise as T and T′—there can be but little doubt that from the point of view of scientific and engineering progress the authors did well to abandon the major portion of their undertaking. Much of the work they originally planned has been excellently done since in other treatises, as, for example, in Maxwell's *Electricity and Magnetism*, Rayleigh's *Sound* and Lamb's *Hydrodynamics*. Had they proceeded with the book as originally planned, there would have been considerable overlapping with these and similar

treatises, and the extra demands on Thomson's time would have seriously crippled his other work.

A passage from Professor Max Born's inaugural lecture as Tait professor of natural philosophy in the University of Edinburgh bears witness to the unique position occupied in its day by Thomson and Tait's treatise. Professor Born said:

"The chair which I have been elected to occupy, in succession to Professor Darwin, is associated with the name of a great scholar of our fathers' generation, Peter Guthrie Tait. This name has been familiar to me from the time when I first began to study mathematical physics. At that time Felix Klein was the leading figure in a group of outstanding mathematicians at Göttingen, amongst them Hilbert and Minkowski. I remember how Klein, ever eager to link physics with mathematics, missed no opportunity of pointing out to us students the importance of studying carefully the celebrated *Treatise on Natural Philosophy* of Thomson and Tait, which became a sort of Bible of mathematical science for us.

"To-day theoretical physics has advanced in very different directions, and 'Thomson and Tait' is perhaps almost unknown to the younger generation. But such is the fate of all scientific achievement; for it cannot claim eternal validity like the products of great artists, but has served well if it has served its time."

Chapter VII

NAVIGATION

LORD KELVIN loved the sea, and his inventive mind often turned to the problems which are of importance to seafaring men. For many years he delighted to escape to his yacht the *Lalla Rookh*, where he found the seclusion necessary for much of his scientific work. He said of himself, " I am a sailor at heart "; certainly in all that concerned the art of navigation he took a very deep interest, and his appliances have added greatly to the security of all those who go to sea. The two oldest aids to navigation, the compass and the sounding line, he practically revolutionized. He has been called the best friend the sailor ever had, and J. A. Ewing relates that a blue-jacket was once heard to remark: " I don't know who this Thomson may be, but every sailor ought to pray for him every night."

Having undertaken to write an article in *Good Words*, in 1873, on the mariner's compass, he began to study the subject seriously. At the same time he was preparing for the Royal Society a biographical sketch of his friend Archibald Smith, containing an account of Smith's work on the theory of the perturbation of the compass caused by the magnetism of iron ships. In 1876, Kelvin took out a patent for an improved compass.

The compass as it then existed had many serious

defects. It was very unsteady, and when the ship rolled it kept swinging through a large angle. An attempt was made to reduce this oscillation by introducing friction at the pivot, but this often made matters worse by causing the compass to stick, while pointing in a wrong direction.

The card was sometimes made heavy and the needles long, under the mistaken idea that this would make it steady. The long needles made it impossible to correct the compass properly for the magnetism of the ship, which was a most serious defect. In iron ships, and especially in ironclads, the compass is at the mercy of disturbing influences which do much to mask the true directive force of the earth's magnetic field. It is necessary to neutralize these. The way to do it is a matter of theory and had previously been pointed out, but it was only through the radical changes in construction which we owe to Kelvin that it became possible to carry the process into effect. Kelvin recognized that short needles must be used, and that for steadiness a long period of horizontal oscillation was necessary, and finally that to keep the frictional error small the weight of the card including the needles must be small. He made the card a mere aluminium rim tied by silk threads to a small central boss, just as the rim of a bicycle wheel is tied to the nave by wire spokes; and from the silk-thread spokes he hung short pieces of magnetized knitting needle to serve as the magnets. The result was that not only was the total weight very small but it was nearly all in the rim, where it is most useful for giving moment of inertia and consequent slowness of period.

Magnets and card together weigh only 180 grains,

for a 10-inch disc, and yet its period of oscillation is much longer than that of the old standard compass, while its friction error is less. Its gossamer structure puzzled navigators accustomed to earlier forms. Prof. J. A. Ewing tells how Lord Kelvin was once showing it to a committee of admirals who felt inclined to say: "Too flimsy, sure to be fragile!" His reply was to throw it across the room. It took no harm, and one version of the story says that he threw after it what was then the Admiralty standard card—a vastly heavier structure—with disastrous results.

Another admirable feature of Kelvin's invention was his method of keeping the compass always level and free from pendulum-like oscillation. He hung the bowl as usual from gimbals, but with knife edges instead of the usual round spindles at the trunnions, and under the card he provided a chamber at the bottom of the bowl partly filled with castor-oil. A glass partition separates the place where the compass card stands from the lower part of the bowl, and in the lower part is the castor-oil. Its function is to damp out any oscillation of the bowl that may tend to be set up by the rolling and pitching of the ship; the viscosity of the oil does this—by dissipating the energy of the swings. At the same time the knife-edge gimbals leave the compass perfectly free to take up a true level. The bowl as a whole, with the gimbals, is hung from springs so as to withstand vibration caused by the action of the screw, or in warships by gun-fire.

The problem of correcting the faults of a compass due to the magnetism of an iron ship is a very difficult one. Her magnetism springs from two causes. There is first the permanent magnetism which is induced by

the earth's field from the time she began to be built; this depends to a great extent on the direction in which her head lay when in the stocks. Then there is the induced part which changes with every change of course. This effect due to the magnetic field of the earth is not entirely transient, nor is the field she acquired in the stocks entirely permanent, but it is convenient to divide the field into two parts permanent and transient. When a ship is "swung", that is, turned so that she heads successively on all points, the permanent magnetism causes an error of the compass of the same nature as that which occurs when a compass needle is placed on a fixed pivot, and disturbed by turning a bar magnet slowly round a vertical axis. The error reaches a maximum twice in a revolution, once to one side and once to the other. Hence it is called the semicircular error.

The permanent magnetism of a ship, as a rule, is not simply in the direction of its length. In general it is inclined both sideways and up and down. Mathematically, we regard it as having three components, one fore and aft, one athwartships and one vertical.

All three components contribute to produce the semicircular error, which is corrected by putting permanent correcting magnets underneath the compass in the binnacle. These are carefully adjusted, so that they produce in the compass itself a horizontal magnetic field which exactly balances the disturbing horizontal field due to the ship's magnetism. The adjustment is carried out by varying the number of magnetized bars and placing them at a higher or lower level in the binnacle so that they act more or less strongly on the compass above.

One group of the corrector magnets faces fore and

aft, and another faces athwartships. The fore and aft magnets are adjusted to correct the error that is found when the ship's head is east or west, the other group is adjusted to correct the error when the ship's head is north or south.

If the ship always remained on a level keel, these two sets of horizontal correctors would suffice to correct completely the deviations which are caused by the permanent magnetism of the ship. But when the ship rolls, or when she is permanently heeled over to one side, another error called the "heeling" error comes in, which arises from the fact that the ship's magnetism has a vertical component. When the ship heels to either side, the component that was vertical to begin with becomes inclined, and a new deviating force comes into play. Suppose, for example, that the ship has been built in England, the vertical part of the permanent magnetism it has acquired in the building will make the bottom part of the hull have polarity of the kind that attracts the north-pointing end of the needle, while the upper works will have polarity of the opposite kind.

We have to consider what will be the effect on a compass standing on the upper deck or on the bridge when the ship heels. The polarity of the bottom of the hull will then give the north point of the compass a pull to the side that is tilted up. The heeling error due to this cause will be a maximum if the ship's head is north or south; it will be zero if the ship's head is east or west. In a steamer, unless there has been a displacement of cargo, there is no continued heel to one side, such as you have in a sailing ship when running on a particular tack, but nevertheless it is important

to correct the heeling error, for as the ship rolls the effect of heeling error is to give the north point of the compass alternate pulls to port and starboard, which tend to set it swinging.

Hence in addition to the horizontal magnet bars which act as correctors of the semicircular error, Kelvin put in his binnacle an upright bar or bars also of permanently magnetized steel, the first function of which is to correct the heeling error so far as that is due to the vertical part of the permanent magnetism of the ship. These bars are put directly under the centre of the compass card. They are adjusted by raising or lowering a can which contains them in the middle of the binnacle.

Thus by a combination of three sets of correcting magnets, two horizontal and one vertical, complete neutralization of the disturbing effect of the ship's permanent magnetization can be obtained. From time to time, if the condition of perfect compensation is to be maintained, the position of these various correctors has to be altered because of the changes which take place in the so-called permanent magnetism of the ship. The navigator has always to be on the look-out for the gradual development of errors from this cause, however perfectly the first adjustment has been carried out.

The next obstacle to be overcome when perfecting the compass is due to induced magnetism in the framework of the ship. It has a long body of magnetizable material which turns in a horizontal plane and is therefore subject to the inductive influence of the horizontal component of the earth's magnetic field. Suppose that we have a pivoted needle and place it above or below a bar of soft iron and slowly turn the bar round in a

horizontal plane. If we neglect the effects of hysteresis, we have to deal only with induced magnetism. When the bar points north or south there is no deflection of the needle, for in these positions the field due to the induced magnetism is in line with the undisturbed earth field. Also, when the bar points east and west, there is no deflection, for in these positions the bar takes up no magnetism. But between these points, when the bar is pointing N.E., S.E., S.W. or N.W., the deflection is at its maximum. So in a ship's compass this error, due to the purely transient magnetism induced by the horizontal component of the earth's field, has its maximum in these four courses, once in each quadrant, and for this reason it was called the quadrantal error.

This disturbing effect is due to the ship being a long body extending fore and aft, and it can be mitigated, or cured, by balancing the excess of fore and aft iron by other iron placed quite near the compass and on each side of it. The two balls which are placed on each side of the binnacle of the Kelvin compass are the correctors for quadrantal error. A suitable size of ball having been selected, their distances from the compass are adjusted until on swinging the ship the quadrantal error disappears.

The possibility of correcting this error of a compass had been pointed out by Sir G. Airy in 1840. But with the old compass card and needles it was impossible, because of the excessive length and large magnetic moment of the needles. To apply the method to a compass of the old pattern would have needed globes of impracticable size, weighing possibly tons. Kelvin, with his short needles and light card, made it possible to carry out the process and so give the world for the

first time a compass that would point truly to the magnetic north.

Another disturbing cause has to be mentioned. The vertical component of the earth's field induces magnetism as well as the horizontal component. It gives rise to an additional error of two kinds, a further semicircular error and a further heeling error. The right way to correct this is to fix a bar of soft iron in a vertical position near the binnacle, so that the magnetism induced on the compass will act as a counterbalance. This bar is known as the Flinders bar, as its use was pointed out by Captain Flinders as early as 1801. In Kelvin's compass it is made in several short lengths of soft iron, which can be put together to make up a bar giving any necessary amount of correcting effect.

There is still a dynamical cause of unsteadiness due to the fact that the point of suspension of a compass card must be placed some way from the centre of gravity to hold the card level against the dipping action of the earth's magnetic field. Consequently every roll to either side applies a mechanical couple tending to set up oscillation, and if the period of the roll were the same, or nearly the same, as the period of oscillation of the card, the disturbance would become so great as to make steering by compass impossible. Kelvin recognized that the right way to obtain steadiness was to make the period much longer than the period of the slowest rolling motion liable to occur in a ship, at the same time keeping the friction as small as possible. In this problem of securing a steady frictionless compass, as in the invention of the mirror galvanometer, his genius for practical dynamics guided him to the right solution.

As an adjunct to the improved compass he invented

the *azimuth mirror*, an apparatus which, standing on the top of the bowl, allows the bearings of distant objects to be readily taken by sighting over the tops of the correcting globes. All this was the work of several years. His first article on terrestrial magnetism and the mariner's compass appeared in *Good Words* in 1874. It was an introductory sketch, and five years elapsed, during which he said he had been learning the subject, before his second article appeared. Hence Ewing says that what he had at first thought would be a " pleasant and easy task " of describing an instrument familiar to navigators for 600 years took five years to accomplish, and before it was completed he had given to the compass a character of precision it had never possessed before.

The evolution of the Kelvin compass took about five years, but, as Ewing has remarked, a longer task lay before the inventor in overcoming the professional conservatism of sailors, the objections of the so-called practical man, active hostility in some quarters, and the passive resistance of official inertia. But gradually the compass became popular, and it had one or two enthusiastic advocates. Foreign Admiralties took it up, and in our own service individual officers were quick to see its merits. Captain Fisher (afterwards Admiral-of-the-Fleet Lord Fisher) was warm in its praise after observing its behaviour in ships under his command, both in rough weather and during the bombardment of Alexandria. That was in 1882. But it was not until November, 1889, that the Superintendent of the Compass Department of the Admiralty was in a position to inform Lord Kelvin that his 10-inch compass was to be adopted as the standard compass for the Navy. This was twelve years after the date of his patent, and more than eleven

years after he had laid the invention before the First Lord. The way of the inventor, like that of the transgressor, may still be hard, but we may trust that it is not so hard now as it was then. As Ewing says, " one does not care to dwell on the spectacle of a Kelvin spending his strength in disheartening effort like the sea beating against a cliff ". In 1910 the use of the Kelvin compass was practically universal. It is painful to read the correspondence and discussions of these weary years, but one does so with increased admiration of the infinite patience which at last secured to us the benefits of his practical genius.

The *navigational sounding machine* was another invention of great importance. His cable-laying experience first led Kelvin to take an interest in deep-sea sounding. The process of sounding the depth was an extremely laborious one. A very heavy sinker was attached to a rope an inch and a half in circumference which offered a great resistance to motion through the water. It therefore took a long time to reach the bottom of the sea. For the same reason the ship had to be stopped while the line ran out and, except in shallow water, while it was being heaved in. Many hands were needed and much time was spent in making a cast. Hence the operation of sounding, beyond the use of the hand-lead in quite shallow water, was little resorted to as an aid to navigation, notwithstanding the importance of the indications it could give when a ship was approaching land in a fog, or in circumstances which made the exact position uncertain. Thomson's theoretical study of the forces acting on a cable during its immersion showed him that in order to make the sounding line slip down quickly, it should have the smallest possible and the smoothest

possible surface. This led him to use a single wire of steel. As the steel used in the wires of pianos was of very high tensile strength, it seemed in every way suitable.

In 1872 he demonstrated the practicability of using this wire, by taking a sounding and finding a bottom at 2700 fathoms in the Bay of Biscay. He used a single wire of No. 22 gauge and a 30 lb. sinker. He soon devised a suitable drum and winding-in wheel for deep-sea use, and from this was developed later a compact form of navigational sounding machine by which flying soundings are taken without stopping the ship.

In a flying sounding the wire streams out behind, taking an oblique course to the bottom, and the length of the wire that runs out is greatly in excess of the depth. To read the depth directly, Thomson invented several forms of depth gauge, the simplest of which is a long narrow glass tube, closed at the top and coated inside with chromate of silver, which is discoloured by the action of sea-water. This tube was put in a protecting case attached near the sinker. As it descends, the increased pressure forces the sea-water up into it, compressing the air and indicating the depth by the height to which the chemical lining is discoloured. The depth is read off by laying the tube against a scale when the line is again drawn on board.

This machine has become a standard navigational appliance. The length of wire in common use is 300 fathoms. A stranded line of seven fine-steel wires instead of the single wire is now employed as it gives it greater flexibility. It runs out under a regulated tension supplied by a rope brake which retards the rotation of the drum on which the sounding line is wound. When the sinker

touches bottom the tension is at once seen to slacken, or rather felt to slacken by a sailor who keeps a rod of wood lightly pressed against the line as it runs out. The drum is stopped and the wire is slowly wound in again by hand, or in the latest naval type by electric motor. Lord Kelvin's latest improvements of the machine were made only a year or so before his death; this was his last serious piece of inventive work.

In the Navy a pair of the Kelvin machines stand on the bridge, and there is a boom at each end along which runs a sounding wire. Whenever soundings are wanted, they can be taken systematically and in quick succession while the ship proceeds at undiminished speed. The depth is called out for the information of the navigating officer almost as soon as the wire has stopped running out. Alike in the Navy and in the Merchant Service there is no difficulty in making it a matter of routine to keep the sounding machines going incessantly when near shore or within, say, fifty fathoms in thick weather.

Another of Kelvin's services to navigation was his advocacy of what is now called the *Position Line* in the working out of a navigator's " sights ". The ordinary " sight " of the sun or of a star is the observation of the altitude above the horizon at a known instant of Greenwich time. The navigator takes its altitude by the sextant, and at the same instant reads the Greenwich time by his chronometer. The Greenwich time tells us that the sun or star was at that instant vertically over a particular spot on the earth's surface, and from the observed altitude we know that the ship was a certain distance from that spot. If the altitude had been 90°, the ship would have been just at the spot in question, and if it were less than 90° she must be some distance

away from it. In fact, she lies on a circle on the surface of the earth or sea from every point of which the altitude would have the observed value. All the sights tell you is that you are on the circumference of a certain circle from every point of which the altitude would have the same value. Practically you are concerned with only a little bit of the circumference—a short arc of it, in the neighbourhood where you know that the ship happens to be. On the chart this little arc can be represented with sufficient accuracy as a straight line; this line is called the position line for the given sight, and once the position line is drawn on the chart you have a complete representation of all that the sight is able to tell, which is that the ship is somewhere on that line. To get the actual position, an independent second observation is required. This may be the bearing of an object on land, or it may be another observation of the altitude of a heavenly body, and if you draw the position line for that also the intersection of the two will be the place of the ship. Both sights may be of the same body—the sun, for example—taken at different times, and if the ship has been moving in the interval, the first line must be shifted parallel to itself through a distance representing the run of the ship, before the intersection of the two lines is used to fix the position—at the time when the second sight is taken.

An American navigator named Sumner was the first to point out the desirability of drawing for every sight the corresponding position line and of showing how the line could be found in practice. But Sumner's method was rather laborious and the advantages of the Sumner line were little understood. Kelvin realized them and saw how the process of drawing the line might be

greatly simplified. For this purpose he published his *Tables for Facilitating Sumner's Method at Sea*, which immediately reduced the labour of calculation and incidentally supplied the navigator working out the sight with a piece of information of great value, namely, the true bearing of the sun or star. Its value is this, that by comparing the true bearing with the bearing taken by compass at the same time as the sight, a test of the accuracy of the compass is incidentally obtained.

The particular method devised by Kelvin did not come into general use, and since then preferable methods have been perfected. But the main point is that navigators have now become familiar with the important truth which Kelvin laid such stress on, that for every sight the position line should be independently drawn.

Kelvin advocated that each fixed light at sea should be identified by making it signal some letter of the Morse alphabet in a group of shorter and longer periods of darkness. Twice also he gave the Admiralty assistance by serving on committees which had to advise as to the selection of types of battleships and cruisers, first in 1871, after the loss of the *Captain*, and again in 1904–5, when the design of the *Dreadnought* was under consideration.

It was at Kelvin's instigation that the British Association formed a committee to investigate tidal phenomena by a method of harmonic analysis introduced by him. Writing to Helmholtz in that year he speaks of having spent many a day on the study of tides on board the *Great Eastern* when waiting for the weather to change or when making passages. In Kelvin's analysis of the tides the constituent terms are selected with reference to the various physically recurring influences which go

to build up the resultant tide and not as in a simple Fourier series where the terms are in an arithmetical progression. The actual tide is treated as the resultant of a group of tides, each due to a physical cause having a definite period of its own, determined by reference to the particular cause to which each constituent tide is due. Thus there is in the first place the lunar semi-diurnal tide due to the moon, the chief of all the constituent tides, whose period is half a mean lunar day. Then there is the solar semi-diurnal tide, whose period is half a mean solar day. Component tides have to be taken into account due to the ellipticity of the moon's orbit round the earth and of the earth's orbit round the sun, &c. Finally, there is the tidal wave in a restricted tidal channel, sometimes the most important constituent which has to be taken into account. This is allowed for in the analysis by introducing the higher harmonic terms of a Fourier series.

The practical utility of the whole process lies in this that it enables the behaviour of the tide at any port to be predicted with great exactness. After observations of the tides have been made for a sufficiently long time, either by systematic measurements of the water level from hour to hour, or by means of a self-recording tide gauge, the analysis can be applied to calculate the phases and amplitudes of the constituent tides, and once that is done it becomes possible, as a mere matter of computing, to work out the future tides for the port.

To facilitate the process, Kelvin invented in the first place a mechanical analyser for getting the constants of the constituent tides out of the recorded readings of tide gauges at the port. Later it was found better to carry on this part of the work without mechanical aid,

namely, by measurement and computation. In the second place he invented a mechanical tide predictor which carries out the subsequent part of the operation, giving a very complete automatic synthesis of the constituent effects, drawing a curve, in fact, which shows for a whole year or longer the future behaviour of the sea-level at any port for which the constants of the constituent tides have been determined. This machine is in regular use at the National Physical Laboratory, for working out the future tides for Indian ports, and the results of the calculations are published in two annual volumes, which give full particulars of the future tides for all the chief ports of the Indian Ocean from the Red Sea to the coast of Burma, and from Suez to Port Blair.

Chapter VIII

ELECTRICAL OSCILLATIONS. VORTEX ATOMS

HELMHOLTZ, in a well-known paper published in 1847 on the conservation of energy, discussed some puzzling results obtained by Riess on the magnetization of iron wires by the current in a Leyden jar discharge flowing in a coil surrounding them. A wire left magnetized had the north pole sometimes at one end and sometimes at the other. A possible explanation of these results is that the discharge is an oscillatory one.

A Leyden jar consists of a glass bottle coated inside and outside with layers of tinfoil, insulated from one another by the glass. When the inner and outer layers are each connected with a terminal of a frictional machine, charges of electricity are induced in the two coatings, and these charges are of equal magnitude but of opposite sign. If the extremity of a wire connected with the outer coating be brought near a wire connected with the inner coating, then, when they are sufficiently close together and the charges are sufficiently large, a spark which makes a loud snapping noise passes, and the quantities of electricity in the two coatings are neutralized, a rush of electric current taking place in the wires.

Helmholtz showed that an electric current cannot

rise instantaneously to a finite value—it requires time. While an electric current flows, there is energy stored up all round it, and the energy of the currents in the discharge wires of a Leyden jar comes from that fraction of the initial charge which has left the jar. This transference of energy cannot take place instantaneously. When the coatings have lost all their electric energy, then, if the wires had no resistance, and no energy was dissipated by the spark, the current in them would be a maximum, and the energy of this current would be exactly equal to the energy originally stored in the coatings. As the current diminishes, this energy is restored to the coatings, which are now charged in the reverse direction, and, when the current stops, the whole energy is stored up again in the jar. It will now discharge in the reverse direction, and the process will continue in a cycle. In practice, energy is dissipated at the spark and in heating the wires, and the oscillations only take place under certain conditions; in any case they get feebler and feebler until there is not enough energy to start the current flowing across the gap.

This phenomenon is analogous to the motion of a pendulum swinging freely. If it is swinging in air the amplitude of the oscillation will gradually get smaller and smaller until it stops. In this case the damping is said to be small. If it were swinging in water the damping would be much greater, and if it were swinging in a heavy viscous liquid like treacle, it would not oscillate at all, but, when displaced, would gradually move back to its middle position without passing it. The resistance of the electric circuit to the electric current is analogous to the viscosity of the liquid. If it is very small, then so

is the damping of the oscillations, and when the resistance exceeds a certain value we do not get any oscillatory discharge at all. Thomson suggested that an experimental verification of this phenomenon might be obtained by means of a Wheatstone's revolving mirror, and this was actually done by Feddersen in 1859. The invention of the oscillograph has enabled the discharge currents to be studied in minute detail, and the mathematical formulæ which Thomson first deduced have been shown to represent accurately what happens during the oscillatory and also the non-oscillatory discharge.

The great practical importance of Thomson's paper, however, lies in an application which at that time was undreamt of—namely, to radio communication. When the oscillations are very rapid, a large amount of the energy stored in the jar can be radiated into space. Hertz showed that the radiation can be detected by means of a device called a detector, consisting of a ring of wire with a small gap in it. If the ring is set with its plane at right angles to the path of the beam, the radiation causes a spark to pass across the gap, and so this device can be used for signalling. A detector of this kind could only be used for short distances. The discovery by Branly in 1890 of another device called a coherer enabled the radiated waves to be detected even at great distances. Further discoveries by Lodge, Marconi and others have brought this system of signalling into everyday use. In consequence of the reflection of these beams or waves by certain portions of the upper atmosphere, it has been found possible to transmit signals round the earth, not only once, but many times.

Thomson's simple and beautiful mathematical theory predicted the oscillatory discharge of a condenser. It enabled the frequency of the oscillations of the waves to be accurately computed and very readily altered and adjusted, and so was an invaluable help to those early pioneers of radio who made such epoch-making discoveries. Not only is radio communication a rival to telephony and telegraphy, both land and submarine, but its rise in conjunction with both is proving of the greatest value. World-wide broadcasting has sprung from the discovery by theory of a physical phenomenon which was at first regarded as only of interest to physicists. Long-distance television also is now beginning to make rapid progress, and we can look confidently forward to seeing it widely used in the future.

Thomson also suggested that as a lightning flash was of the same nature as a Leyden jar discharge, but only on an enormously greater scale, it was highly probable that a lightning flash is an oscillatory phenomenon. It has been observed that a lightning flash apparently lasts for an appreciable time. It would be easier to explain this appearance, if the discharge were an oscillatory one.

To Nichol's Cyclopædia, published in 1850, Thomson contributed the article on *atmospheric electricity*. This, with many other articles describing his electrometers for measuring and recording the potentials of the air at various points in the atmosphere, with the results of experiments, are given in the *Reprint* of his papers published in 1872.

He states that he has sometimes observed extremely rapid variations of terrestrial electrification on his instruments, although the weather has been very calm. Large variations occurred even in a minute of time.

He thought it probable that these variations in the observed electric force were due chiefly to positively or negatively electrified masses of cloud passing within a few miles of the locality of the observation. If a cloud be electrified, the masses of air it contains may not be at the same potential and, if the potential gradient between them be sufficiently high, there may be a discharge, possibly oscillatory, which will tend to equalize their potentials. These discharges would be noted by a suitable electrometer even at a considerable distance away. If we have a cloud electrically charged, then we can have flashes inside the cloud from positively to negatively electrified regions, or we can have discharges from the upper part of the cloud to the upper atmosphere. In tropical thunderstorms, discharges inside the cloud predominate. In temperate regions the clouds are lower and so earth flashes are more frequent.

By means of Boys' rotating camera which can be rotated at a speed of 3000 revolutions per minute, excellent photographs can be obtained of ordinary and oscillatory flashes. In this way Schonland and others have obtained very instructive photographs.

It appears that each flash is initiated by a "leader stroke" which shoots downwards from the cloud to the earth. This stroke draws from the cloud a considerable charge which is distributed along its channel. After an interval of the order of a microsecond, there is a powerful return stroke (the main stroke) to the cloud, this stroke using the channel which had been blazed by the leader stroke. The return stroke carries the large currents of thousands of amperes usually associated with the lightning flash.

Sometimes the leader stroke is extinguished before it

has travelled more than a fraction of the total path to earth. About the hundredth of a second later a second streamer may follow on the same track as the first streamer and blaze it a little farther, possibly in a new direction. Subsequent similar streamers may follow until the path reaches the earth, when the main stroke from earth to cloud takes place. In this case the path is said to be stepped.

T. E. Allibone has published in the *Journal of the Institution of Electrical Engineers* for 1938 the results of an experimental study of the mechanism of a long spark between various shaped electrodes caused by impulse voltages, that is, voltages caused by the discharges of batteries of condensers. In the *Proceedings of the Royal Society* for the same year, in conjunction with J. M. Meek he contributes a paper on the development of the spark discharge and applies his results to lightning discharges. Both papers show photographs of discharges taken by a rotating camera.

Let us consider what takes place between a spark-gap, consisting of a point and a plane, when a gradually increasing impulse voltage is applied to it. Discharges are first observed preceding the main spark, irrespective of the polarity of the high-voltage electrode. These pre-discharges are called " leader strokes "; when one of these reaches the other electrode, we have the return stroke or the main stroke of the discharge. Under certain conditions leader strokes have been shown to develop from both electrodes simultaneously. The leader stroke always exhibits branching in the direction of propagation. The main stroke follows some of the more important branches, but does not develop fresh branches.

When the shapes of the electrodes differ widely as, for example, in an air-gap consisting of a point and a plane, discharge usually occurs first at the smaller electrode, whether it be the cathode or anode or whether it be earthed or connected with the high potential source. Faraday pointed out that the sparking potential is usually lower when the smaller electrode is positive.

It is to be noticed that there is no return stroke at all until the leader stroke has bridged the gap between the two electrodes. It starts from one electrode and then ceases after going a part of the distance. A short time later a second discharge starts from the electrode, traverses the path taken by the first discharge and then proceeds to blaze a new track over another section of the gap. The leader may on this or on a subsequent effort make contact with the opposite electrode. When this happens there is a thinly ionized path between the two electrodes, and provided that the voltage difference is still maintained, a great increase in ionization takes place, starting from the electrode to which the leader stroke had been directed; or, in the case of a double leader stroke, it starts from the junction point of the two leaders. In either case it proceeds to the opposite electrode. Even with point-to-point discharges this often happens. The resemblance of what happens during a lightning discharge with what happens with a " long spark " between electrodes is noteworthy.

It seems probable that as knowledge progresses, lightning flashes will be divided into several different groups, depending on the climate and the region of the earth where the storm is taking place, and on whether the " ground " is positively or negatively electrified. A study of Kelvin's writings shows how often he

considered these problems and what a boon rotating cameras, cathode-ray oscillographs and high-voltage laboratories would have been in helping him to unravel the mysteries of the electrification of the earth's atmosphere.

The motion of vortices in liquids was a problem which keenly interested Thomson. In *Crelle's Journal* for 1858 there appeared a classical memoir on the subject by Helmholtz, in which he states with admirable clearness the main laws which govern their motion. Thomson took up the investigation where Helmholtz left it and developed it much farther, with the object of making a complete mechanical theory of the æther based on vortical atoms.

The motion of a whirlpool or a whirlwind is an example of a simple vortical column with two ends; a smoke ring is an example of a circular vortex closed on itself. If we draw a semicircular plate rapidly through the surface of water for a short distance, we get a semicircular vortex with its ends on the surface. P. G. Tait invented a method by means of which large smoke rings containing a considerable amount of energy could be produced with ease and certainty, and Thomson used this method in his class to illustrate the properties of vortex rings.

When two smoke rings hit one another they rebound, shaking violently from the effect of the shock, just as if they were made of rubber. A very curious phenomenon is observed when two co-axial vortex rings are moving in the same direction. The leading vortex ring dilates and moves more slowly, but the lagging vortex contracts and moves more rapidly. The two rings apparently attract one another, hence the lagging ring overtakes

and passes through the first. The same actions take place again, their rôles being now reversed. These actions can easily be observed with tobacco smoke rings or with the half vortex rings made by drawing a semi-circular plate rapidly through water.

When a vortex ring approaches a wall it expands. At the same time its translational velocity slows down, but the rotational velocity of the molecules composing it increases. Thomson explains this by the method of images. We imagine the wall removed and an equal vortex, the image of the first, to be approaching it. The mutual action between them is repulsive; therefore their speed slows down and it can be shown by dynamical principles that they must expand.

If viscosity, that is, fluid friction, could be neglected, a vortex once started would exist for ever. This suggested to Thomson the idea that the only true atoms, of which the universe is made, are vortex rings.

Thomson believed that the hard, impenetrable, spherical atom of Democritus and Lucretius was a highly improbable assumption. In years past it was perhaps the only assumption capable of explaining the unalterable distinguishable qualities of different kinds of matter, but now that Helmholtz had proved that the strength of a vortex filament moving in a perfect fluid remains constant, the Lucretian hypothesis could be abandoned. Vortex rings in a perfect fluid would exist for ever. To generate them can only be the action of a creative power.

The results obtained by spectrum analysis prove that the ultimate constituents of simple bodies must be capable of vibrating in one or more different ways. In this respect they must be like a stringed instrument,

having one or more strings, or a solid consisting of one or more tuning-forks rigidly connected. Thomson pointed out that on the Lucretian hypothesis we must suppose that the molecule of sodium, for example, consists of a group of atoms with void spaces between them, because a single, infinitely hard spherical atom could not have one, and most certainly could not have two free periods of vibration. Hence it loses the one recommendation which inclined philosophers to accept it provisionally.

Thomson proved that the vortex atom has perfectly definite periods of vibration, which depend solely on the motion which constitutes it. He thought it probable that the atoms of substances did not consist merely of vortex rings, but consisted of two or more linked together like the links of a chain. It is easy to see that a vapour consisting of such atoms would be capable of satisfying the very exacting spectrum test. This is a great advance on Newton's corpuscular theory, as it is capable of explaining very abstruse phenomena. When, during Newton's life-time, the many arbitrary assumptions he had to make about his corpuscles, in order to explain various phenomena, seriously discounted the value of his theory. Thomson was at first inclined to adopt the elastic solid view of the æther, but this he abandoned when he saw that the necessary rigidity could be obtained more simply by imagining an æther formed of vortex rings.

Modern scientific men are inclined to believe that matter itself is but an ætherial manifestation. Larmor in the following passage states this clearly, and indicates some of the difficulties connected with the vortex atom theory.

"The fluid vortex atom faithfully represents in many ways the permanence and mobility of the sub-atoms of matter; but it entirely fails to include an electric charge as part of their constitution. According to any æther theory, static electric attraction must be conveyed by elastic action across the æther, and an electric field must be a field of strain, which implies elastic quality in the æther instead of complete fluidity; the sub-atom with its attendant electric charge must therefore be, in whole or in part, a nucleus of intrinsic strain in the æther, a place in which the continuity of the æther has been broken and cemented together again (to use a crude but effective image) without accurately fitting the parts, so that there is a residual strain all round the place."

One cannot say for certain what Kelvin's attitude would have been to the theories of matter and space which have arisen during the present century. It is probable that he would have been most unsympathetic. To him the æther was something very real, and the determination of its nature one of the most important as well as most fascinating of the problems of physics. To abolish the æther entirely and replace it by a vague electromagnetic field would in all likelihood have seemed to Kelvin not to be solving the problem but to be giving it up!

CHAPTER IX

HEAT, TEMPERATURE AND WORK

IN 1880, Thomson wrote for the *Encyclopædia Britannica* (9th edition) a long article on Heat, on which he had obviously bestowed much care and labour. On many points he found that trustworthy data were not obtainable, and he expended a great deal of time in finding by experiment the results which were required. Hence the article is largely an original research on the subject.

He begins by saying that " heat is a property of matter which first became known to us by one of six very distinct senses ". He points out that the sense of touch, as commonly understood, has two distinct objects—force and heat. When a person stretches out his hand till it meets anything solid, or holds it out while something solid is placed upon it, he experiences a sensation of force. In the one case he perceives resistance to the previous motion of his hand, in the other the necessity of resisting to prevent his hand being forced downwards. In each case the immediate object of this perception is force. But there is another very distinct sensation, namely, heat or cold, and this may be perceived even when no sensation of force is experienced. If the person performs the operation with his eyes shut, the " double " sense of touch informs his mind that his hand has met either a hot fixed or a cold fixed body. We now call " heat " the property of matter

concerned in these sensations, and "temperature" a certain quality of matter which varies when heat is communicated to it from, or taken from it to, external matter.

In special cases temperature does not vary in a mass of matter when heat is given to it or taken from it. For example, when the body is ice at its melting-point, heat communicated to it does not alter its temperature; and the same is true if the body be water at the freezing-point with some ice in it. Thomson divides heat into "sensible" heat and "latent" heat. Heat given to a substance and warming it is said to be sensible in the substance, and heat given to a substance and not warming it is said to become latent.

The doctrine of latent heat leads us to a very important measurement in thermal science, namely, the measurement of a quantity of this wonderful property of matter which we call heat. It is not necessary to know what the nature of heat is in order to measure it. It may be a light elastic fluid or a state of motion, or possibly a modification of the state of matter related to the action of force. We may therefore agree to measure quantities of heat by quantities of ice melted into water without any change of temperature. Thus if a kilogram of ice is melted by a large quantity of water at a lukewarm temperature or by a comparatively small quantity of water at a high temperature, the same quantity of heat has certainly gone from the warm water to the ice in each case, supposing that the result in each case is that the ice and the warm water are left finally in a state of ice-cold water. The measurement of quantities of heat, whether by the melting of ice or otherwise, was called calorimetry in the days when the essence of

heat was supposed to be a fluid called caloric. The name calorimetry is still retained to designate measurements of quantities of heat.

The sense of heat and cold is not simply dependent on the "temperature" of the body touched. If a person takes a piece of iron or a stone, or a piece of wood, or a ball of worsted, or a quantity of finely carded cotton-wool, or of eider-down in his hand, he will perceive the iron cold, the stone less cold than the iron, the wood only slightly cold, and the wool and the down decidedly warm. We know that if all the bodies were near one another and similarly exposed, they must have been at the same temperature, and since the iron and stone feel cold we know that this temperature is lower than the temperature of the hand. The iron being a good thermal conductor, keeps drawing off heat from the hand and lowering its temperature until after some time an approximately permanent temperature is reached considerably lower than the original temperature of the hand. When wood is touched, its small conductivity allows the surface to be warmed again after the first few seconds, sometimes even to a higher temperature than that before contact; and thus if the sensation could be perfectly remembered, it would be perceived that the wood was first felt to be cold and afterwards to be warm.

The sense of heat is in reality a somewhat delicate thermal test when properly used. Even an unskilled person, if he dips his hand successively into two basins of water, will detect a difference of temperature of less than a quarter of a degree Centigrade.

Without knowing anything of the nature of heat, we might devise a complete system of thermometry by mixing hot and cold water and using our sense of heat,

Lord Kelvin's Laboratory

if we had two definite constant temperatures of reference. In practical thermometry these are supplied, by the melting-point of ice and the temperature of steam from water boiling in air at a definite pressure. Thomson states that this method can be trusted within two-tenths of a degree Centigrade, and gives more accurate results than many common thermometers sold for ordinary purposes. This system, however, would be strictly limited to the range between the freezing- and the boiling-points of water (0° C. and 100° C.).

Thomson goes on to describe liquid thermometers, the liquids being mercury, alcohol, &c., and a complete series of steam pressure, sulphurous acid, and water thermometers adapted to give absolutely definite and highly sensitive thermometric indications over a much wider range than the Centigrade scale, which only gives the temperature between the freezing-point and the boiling-point of water—0° C. to 100° C. Thomson's range of thermometers read from something much below −30° C. to considerably above 520° C.

In getting data for his article he used thermometers in which the indicating substances were saturated vapours of different substances including sulphurous acid. Practically everyone in his laboratory, while this research was in progress, was employed in making sulphurous acid in the usual way by heating copper in sulphuric acid and condensing the gas in tubes immersed in freezing mixtures. The atmosphere of the room, although possibly noxious to germs of various kinds, made breathing rather difficult. One morning when all were thus occupied, an eminent chemist, who had just come from the south for a vacation, called to pay his respects to the professor. After a word or two of

inquiry as to how his young friend was prospering in his new post, Thomson said: "We are all very busy brewing liquid sulphurous acid, for use in sulphurous acid steam thermometers; we want a large quantity of the liquid; would you mind helping us?" So, desiring an assistant to find a flask and materials, he enrolled this new and excellent recruit on the spot; and what was intended merely as a call, was prolonged into a long day of ungrudging work at an elementary chemical exercise.

Ordinary thermometers are founded on some particular property of a particular substance. A thermometer graduated to fulfil one of the definitions for one particular substance would not agree with another thermometer graduated according to the same definition for another substance, or according to some of the other definitions. A much more satisfactory foundation for thermometry is afforded by thermodynamic science. It gives us a definition of temperature depending on certain thermodynamic properties of matter in such a manner that if a thermometer is graduated according to it from observation of one class of thermal effects in one particular substance, it will agree with a thermometer graduated according to the same thermodynamic law from the same class of effects in any other substance. Thus we have what is called the absolute thermodynamic scale. In modern science this is the ultimate scale of reference for all thermometers of whatever kind.

In 1847, Thomson heard Joule read an abstract of a paper on the Mechanical Value of Heat at one of the meetings of the British Association. He was so impressed by this paper that, without waiting for an

invitation, he rose and pointed out to the meeting the great interest of the new theory and its practical importance. This was the beginning of a lifelong friendship between the two great physicists, and they jointly carried out an extensive series of experiments on the heat effects of fluid motion.

The epoch-making idea introduced by Joule, and demonstrated by many exact experiments, was that heat and work are mutually convertible in a definite numerical ratio. Before Joule's papers, and indeed for years afterwards, physicists believed that heat was indestructible.

For many years Thomson worked assiduously to perfect the theory of the ideal heat engine on the lines laid down by the great French engineer Carnot, modified by the new idea introduced by Joule. This engine takes a quantity of heat from a source (or boiler) maintained at a constant high temperature, converts part of this heat into work, and then ejects the remainder of the heat into a condenser (or refrigerator) kept at a constant low temperature. In order that this engine be ideally perfect, Thomson saw that it must be reversible; that is, it must be able theoretically to take a quantity of heat from the condenser, and after the expenditure of a definite amount of work on the engine, it must be able to eject into the boiler both the original heat and the heat equivalent of the work expended. He recognized that the amount of work that has to be done depends not only on the difference of temperature between the source and the condenser, but also on the absolute values of these temperatures. For example, the efficiency of a perfect heat engine working between the temperatures of 200° C. and 100° C. is not the same

as that of a perfect heat engine working between 100° C. and 0° C., but is decidedly less.

Thomson was thus led to invent his absolute scale of thermometry. The absolute temperatures of the source and the condenser are proportional to the respective quantities of heat taken from the former and ejected into the latter, during a cycle. If these quantities are measured, the relative magnitudes of the absolute temperatures can be found, and hence also, by measuring the difference of temperature between the source and the condenser, the zero from which they are reckoned can be determined. This scale of thermometry is quite independent of the physical properties of the working substance of the ideal engine. It is known as Thomson's (Kelvin's) absolute thermodynamic scale.

Having found a true absolute scale to measure temperature, the next thing he had to do was to find the relation between this theoretical scale and some easily constructed practical one. A gas thermometer would be a suitable standard provided that a gas which exactly obeys the laws of Boyle and Charles can be found. Boyle's law is that when the volume of a given quantity of gas is doubled, its temperature remaining the same, the pressure is halved, and Charles's law is that the volume of a given quantity of gas increases by a definite fraction of its volume for each degree of rise in temperature. Thomson and Joule, therefore, carried out many tests to find out how far any given gas obeys the ideal conditions.

The method of experimenting devised by Thomson was to force the gas in a steady current through a porous plug and observe very accurately the temperature of the gas on the two sides of the plug. In the case of most

of the gases that he and Joule examined, a slight cooling effect of the gas was produced. It follows theoretically that the absolute zero of temperature on the Thomson (Kelvin) scale is slightly higher than would be found by thermometers constructed by utilizing the expansions of those gases to define a scale. With carbonic acid gas the cooling effect was much greater than with air, nitrogen or oxygen. This might have been anticipated, as Regnault had previously found that the relative increase in volume for a given rise of temperature is greater for carbonic acid gas than for the constituents of air. They also found that the higher the temperature of the gases on which they experimented, the smaller was the cooling effect. Hence at high temperatures the dilatation of the gas was more accurately proportional to the temperature on Kelvin's scale than at low temperatures.

With hydrogen, however, they found that a contrary effect was produced. When this gas was used a slight heating effect was produced by forcing it through the plug. The experimental investigation of the reasons for these small differences in temperature proved to be very difficult and laborious. The experiments were carried out in Manchester, but were cut short by the action of the owners of adjacent property, who threatened Joule with legal proceedings owing to the vibration and noise which were produced.

The results of these experiments were communicated in a series of papers to the Royal Society. They proved that the temperature of melting ice was 273·7° on Kelvin's scale (273·7 K.) and that the temperature of boiling water was 373·7 K. The experiments show that when a gas expands at constant temperature it absorbs

an amount of heat the mechanical equivalent of which is very nearly the same as the work done during the expansion. In 1842, Mayer of Heilbronn assumed as a self-evident proposition that the work done was the exact mechanical equivalent of the heat absorbed, and hence calculated the mechanical equivalent of heat. If his data had been accurate, this would have given a good approximation to the true value, but the assumption he made could only be justified by experiment. Joule and Thomson's experiments prove that, to a rough approximation, the assumption is justified, and they also determined approximately its limitations.

Many interesting incidental phenomena are discussed in these papers, as, for example, the effect of fluid friction in drying steam issuing from a high-pressure boiler. Clausius and Rankine (Thomson's colleague at Glasgow) had independently made the discovery that when steam is allowed to expand heat must be added to it if it is to remain dry steam. It is difficult to reconcile this with the fact that when steam escapes from a high-pressure boiler into the open air through a small aperture it remains dry. Thomson explains this by taking into account the heat developed by the fluid friction of the steam rushing through the aperture; the heat thus communicated to the escaping steam keeps it dry. Hence high-pressure steam sometimes produces a much smaller scalding effect than low-pressure steam.

Another question discussed was: does a mercury thermometer placed in a strong draught of air read the true temperature of the air? The authors found that it will read a little too high. The explanation is that the retardation of the air flowing past the bulb makes it lose part of the energy due to the motion. This lost

energy is converted into heat, and some of it goes to raise the thermometer reading. If the thermometer be sheltered from a gale by being placed near the top of a wall which is at right angles to the direction of the wind, and if the air round it is at rest, then the thermometer will indicate a higher temperature than when placed in the blast. The explanation is that the heating of the air by friction near the top of the wall is on a large scale and affects the thermometer appreciably.

Thomson accepted the theory of the molecular structure of bodies. However homogeneous a substance appears to be, if a portion of it were magnified sufficiently, it would be seen to consist of molecules, and thus it cannot be really homogeneous. At ordinary temperatures, also, it would be seen that the molecules are in motion. The heat of the body is the energy of motion of its molecules. At the absolute zero of temperature these molecules would be at rest, and the heat contained in the body would be zero. Looked at from this point of view, we see at once why there must be an absolute zero of temperature, and also why the properties of bodies change as we cool them down. For example, we have now very strong experimental evidence for saying that at the zero of temperature the electrical resistance of a wire would be absolutely zero. Hence, millions of horse-power could be transmitted from any part of the world to any other, or even from one planet to another, by a thin wire, provided that its temperature could be maintained at absolute zero.

To enable his students to picture the electrical constitution of solid, liquid and gaseous bodies respectively, Thomson used to give the following illustration. Imagine a harbour full of small boats, so tightly wedged together

that they all kept the same relative places. The waves would cause the boats to rub together, and the bigger the waves the more violent this action. In this case the boats would represent the molecules of a solid. At the zero of temperature they would be at rest; at ordinary temperatures every molecule would be vibrating. If the boats were loosely packed together, so that they could drift relatively to one another, but yet always be in contact with many of their neighbours, this would represent the molecules in a liquid. Any given boat might drift from one part of the harbour to another, but it would always be in contact with other boats. Finally, if the number of the boats was so few that they drifted about by themselves over considerable distances before they came into collision and bounded off from another boat, they would represent the molecules of a gas.

In a gas, therefore, the path of a molecule would consist of broken straight lines; in a liquid it would be a curve, and in a solid it would merely be a vibration to and fro about a fixed point.

In 1884, Thomson read an interesting paper "On the Efficiency of Clothing for Maintaining Temperature". He showed experiments which proved conclusively that, if bodies are below a certain size, the effect of putting a covering round them is sometimes to cool them. For example, if we have a bare wire, parts of which are surrounded by transparent substances like glass and mica, and send an electric current through it, if the current be great enough the bare parts of the wire become incandescent, but the parts covered by the transparent substances are quite dark. This proves that the covered portions are the cooler. The reason of this is that the convection currents of air streaming round

the conductor carry away more heat from the covered than from the uncovered portions of the wire, owing to their greater diameter. Hence the heat generated in the covered portion of the wire is carried away more quickly and its temperature is therefore lower. This effect has important practical applications in connexion with the "lagging" of steam pipes and with electric power transmission along covered wires.

CHAPTER X

SCIENTIFIC SPECULATIONS

IN 1862, Thomson published two epoch-making papers; one was on the secular cooling of the earth and was published in the *Transactions* of the Royal Society of Edinburgh; the other, on the age of the Sun's heat, was published in *Macmillan's Magazine*. Both papers were considered so instructive that they were included as appendices in Thomson and Tait's *Natural Philosophy*. Although mathematical physicists eagerly welcomed them, geologists and biologists thought them unconvincing. Huxley was the protagonist of the latter class, and his attack elicited a brilliant and spirited rejoinder from Thomson.

Hutton, Playfair and Lyell taught what Thomson called the doctrine of eternity and uniformity in geology as opposed to the cataclysmal doctrine, which assumed great disturbances and tremendous differences of climate in past ages. Playfair, the brilliant advocate of Hutton's theory, says that in the planetary motions we discover no mark either of the commencement or termination of the present order of things. Thomson totally disagreed with this. He pointed out that Newton in his *Principia* says that planets and comets keep their motions a long time because the space in which they move offers little resistance to motion. The motion of comets, however, proves that it offers some resistance, and hence

changes are continually taking place in the solar system. Laplace's nebular hypothesis and other astronomical theories have a cataclysmal basis, and this agrees with the common view. Pope, for example, says:

> "Who sees with equal eyes as God of all,
> A hero perish, or a sparrow fall,
> Atoms or systems into ruins hurl'd,
> And now a bubble burst and now a world."

The great Charles Darwin demands hundreds of millions of years in his geological periods. It is of importance, therefore, to study Thomson's reasoning, and see how far he has been successful in putting limitations to the periods of time demanded by geologists.

It is universally admitted that at one period of time the earth must have been a rotating molten mass. If we assume that this mass is practically homogeneous, it is not difficult to calculate the shape it would assume. It would be approximately spherical, the poles being flattened and the equator protuberant. Now, when the earth solidified, it would probably retain the same shape as it had when molten; but this shape depends on the velocity of rotation, and hence geodesic measurements enable us to compute this velocity. The polar diameter is known to be 26·7 miles less than the equatorial diameter, and on the given assumptions it can be shown that this is the shape that a liquid mass the size of the earth would have if rotating at its present rate. If the liquid mass were rotating at twice this rate, it would be distorted four times as much from the shape of a sphere. In addition, if it cooled when rotating at this doubled rate, it is highly probable that all the continents

would have formed a dry belt round the equator, and that the poles would be the central points of the polar oceans. The mere fact that we have no dry equatorial belt seems to prove that the effect of the solidification of the earth on its velocity was limited.

Immanuel Kant (1724–1804), the great German philosopher, was the first to show that the tides must act as a brake on the earth's rotation. Adams and also Thomson and Tait calculate that the time of the earth's rotation increases by twenty-two seconds every century. Taking this figure, we see that 7200 million years ago it would have been rotating twice as fast. Its shape, therefore, proves that it would not have solidified at this period.

Taking into account all the uncertainties in the figures and calculations, Thomson concludes that 5000 million years ago the earth was certainly not solid, and that probably it was not solid 1000 million years ago. The results, although only roughly approximate, cannot be lightly disregarded and they definitely rule out the immense periods sometimes given.

Another method of determining the age of the earth is to find the rate at which it is cooling, and then after making certain assumptions about the interior of the earth, to use Fourier's mathematical method and calculate backwards to the time when the earth was molten. Thomson loved the mathematical methods used by Fourier and did his best to show his class by means of easy arithmetical examples how useful they were.

From a survey of underground temperatures in different parts of the world Thomson concluded that on the average the temperature of the earth increases

by about one degree Fahrenheit for every fifty feet of descent. Heat, therefore, must be continually flowing from the earth's interior to its surface, and, since the earth's surface does not get hotter, there must be a continual loss of heat from year to year from the surface. The earth, therefore, must either be getting cooler from age to age, or some temporary dynamical action inside the earth must be keeping up the heat. Thomson considered it proved that there was less volcanic energy in the earth now than there was a thousand years ago —just as a battleship has less ammunition on board after it has been discharging shot and shell for several hours.

The chemical hypothesis to account for underground heat would be probable if the rise of temperature as we go downwards occurred only in isolated localities. The suggestion that there might be some slow uniform combustion going on at a great depth under the surface he thought highly improbable. Poisson's hypothesis that the highest underground heat is due to a passage, at some former period, of the solar system through hotter stellar regions is only tenable if there was a well-marked period of discontinuity in palæontology. Thomson calculates that if this passage took place between 1250 and 5000 years ago, then in order to account for the present underground temperature gradient the temperature of the supposed stellar region would have to be from 25° to 50° F. hotter than the present mean temperature. Hence there would have been plenty of evidence available of such a phenomenon. The further back we place the passage of the earth, the hotter the stellar regions would have to be. If the passage took place more than 20,000 years ago, the excess of tem-

perature would have been greater than 100° F., and so animal life would have been destroyed. As no evidence has ever been adduced to support this, the hypothesis is untenable.

Following Leibnitz, Thomson assumes that the earth was once an incandescent liquid sphere. Assuming that the heat constants of this mass are the same as the constants he and J. D. Forbes found by experiments on rocks from a quarry near Edinburgh, and making allowances as to his data, he concludes that the period of time since this earth solidified lay between 20 and 200 million years.

After the crusting over of the earth's surface the terrestrial heat would have little direct influence on climate. After 40,000 years the rise of temperature as we bore downwards would be about one degree per foot; after 100,000 years it would be half a degree per foot; and after 100 million years it would be the fiftieth part of a degree per foot, which is the temperature gradient at the present day:

"Is not this, on the whole, in harmony with geological evidence rightly interpreted? Do not the vast masses of basalt, the general appearance of mountain ranges, the violent distortions and fractures of strata, the great prevalence of metamorphic action (which must have taken place at depths of not many miles, if so much) all agree in demonstrating that the rate of increase in temperature downwards must have been much more rapid, and in rendering it probable that volcanic energy, earthquake shocks and every kind of so-called Plutonic action have been, on the whole, more abundantly and violently operating in geological antiquity than in the present age.

Thomson imagined that the interior of the earth was like a honeycombed solid, the liquid always tending to work its way up, owing to its lower specific gravity. The actions that would take place in such a mass would be sufficient to account for geological phenomena like earthquakes, subsidences and upheavals and eruptions of melted rock. The oceans on the surface of an earth built up in this way would exhibit the phenomena of tides.

In 1899, Kelvin, having the advantage of more accurate data, stated that he agreed with Clarence King in thinking that the time since the earth was molten was about twenty-four million years. This estimate, however, is generally considered nowadays to be much too small. John Perry pointed out that if the conducting power of the material near the centre was greater than that of the material near the surface, Thomson's estimate would have to be raised very considerably.

The discovery of radioactivity has put the whole question in an entirely different position, and throws grave doubt on the possibility of determining the age of the earth solely from the known temperature gradient of its crust. R. J. Strutt (the 4th Baron Rayleigh) has detected radium in many rocks of the earth's crust in sufficient quantity to account for the temperature gradient without the necessity of making any hypothesis about heat being conducted from the interior. The energy given out by the radioactive substances distributed through the soil and rocks doubtless plays an important part in maintaining the earth's supply of heat. Grimsehl computes that if we assume that the distribution of radioactive material is uniform through-

out the whole globe and the same as is observed near the surface, then we find that the rate of evolution of heat by radioactive disintegration would be greater than the actual rate of loss by radiation into space. In order to explain the steady thermal state of the earth, it is sufficient to assume the existence of a relatively thin surface layer containing radioactive elements of the required concentration.

We may assume, therefore, that the concentration of radioactive elements must decrease rapidly with increasing depth. The chemical properties of uranium and thorium indicate that they may have become concentrated in the outermost regions as the earth's crust solidified. Taking mean values as a basis, the following results are obtained for the rate of production of heat per cubic cm. of rock: from uranium and its products 4.0×10^{-13} calories per second, and from thorium 3.7×10^{-13} calories per second. This gives a total of 7.7×10^{-13} calories per second. Knowing the mean thermal conductivity of the earth's substance and the temperature gradient towards the centre, we can calculate that the earth radiates 6×10^{12} calories per second into space. To balance this loss it is necessary to assume the existence of about 4.5×10^{20} grammes of thorium and 1.2×10^{20} grammes of uranium. If, however, the distribution of thorium and uranium were the same throughout the whole globe as it is on the surface, we should have 12×10^{22} grammes of thorium and 3.6×10^{22} of uranium. From this we can compute that a surface layer only about 16 kilometres thick would suffice to maintain the thermal state of the earth. Recently a determination has also been made of the rate of evolution of heat in the radioactive disintegration of

potassium, and it is found that this rate is probably as great as that due to uranium and thorium put together. The numbers, therefore, vary appreciably in these two cases.

The theory of radioactivity is of importance in geology because it provides methods for calculating the age of minerals and rocks belonging to different geological periods. It is assumed that during their time of production the minerals in question have been free from disturbing chemical influences. The calculation may be carried out in three main ways. According to the theory of radioactivity, a mineral containing radium must also contain the non-active end-product, radio-lead. The older the mineral the greater must be the ratio of the lead to the uranium in it. Again, the helium produced in the disintegration is usually retained in the mineral, and the helium content must be proportional to the age of the mineral.

The third method makes use of the small haloes sometimes found in natural crystals. These are a result of bombardment by the alpha particles from minute radioactive inclusions over long periods of time. They often show clearly marked rings corresponding to the different ranges of the alpha particles from successive members of a disintegration series. These haloes can be imitated artificially.

At present these methods can only be considered provisional; they are useful for determining roughly the ages of the various geological epochs. The general results found in these ways are satisfactory, as the ages deduced are of the same order of magnitude and in the same ratio. Grimsehl finds the numerical values to range from about 8 million years for the Oligocene

period, 31 million for the Eocene, 150 million for the Carboniferous, 300 million for the Devonian (first land plants) to about 1000 million for the Precambrian. It is concluded that the first appearance of life on the earth was probably about 1200 million years ago, and an age of 1600 million years has been suggested for the oldest gneissoid granites. These values are consistent with estimates based on independent geological considerations.

Chapter XI

FELLOW SCIENTISTS

Sir George Gabriel Stokes.

AMONG his scientific contemporaries, there was none whom Kelvin admired more, or had a greater affection for, than George Gabriel Stokes. The son of a Church of Ireland clergyman, and a few years older than Thomson, Stokes entered Pembroke College, Cambridge, in 1837, and became senior wrangler, first Smith's prizeman, and fellow of his college in 1841.

Soon after coming up to Cambridge, Thomson became acquainted with Stokes, and before long they were close friends, a relationship which only grew stronger as the years passed. When Thomson in 1845, immediately after completing his undergraduate course, took over the editorship of the *Cambridge Mathematical Journal*, it was Stokes's papers, along with his own, which during the next ten years made this periodical famous in mathematical and physical circles throughout Europe.

In 1849 Stokes was appointed Lucasian professor of mathematics at Cambridge, and held this post till his death in 1903. During his tenure of this chair, more especially in the earlier years, Stokes's papers earned him a reputation second to none among the mathematical philosophers of the day. Thomson was once asked who was the most outstanding physicist on the

Continent. He replied, "I do not know, but whoever he is, I am certain that Stokes is a match for him." Thomson frequently referred to the immense benefit he had derived from his friendship with Stokes. At Stokes's jubilee as a professor, one of Kelvin's remarks was: "Whenever a mathematical difficulty occurred to me, I used to say to myself, 'Ask Stokes what he thinks of it.' I got an answer if answer was possible; I was told, at all events, if it was unanswerable. I felt that in my undergraduate days, and I feel it more now."

Almost as strong was Stokes's reliance on Thomson. "Consult Stokes" was paralleled by "What will Thomson think?" For more than fifty years they were in constant communication, each imparting to the other his new ideas as they occurred to him. One result was that in after years they had sometimes difficulty in deciding with which of them a new idea had started. This was so, notably, with the fundamental principle of spectrum analysis, afterwards developed into a scientific method by Bunsen and Kirchhoff. During a visit to Paris in 1850, Thomson had seen the striking demonstrations by Foucault of the spectra of metals obtained by means of the electric arc. Foucault showed the black absorption line obtained, when the light from the arc was passed through sodium vapour, this black line occupying the same position in the spectrum as the well-known bright yellow sodium emission line. This experiment interested Thomson greatly, and he soon afterwards brought it up in conversation with Stokes. To one or other of them, perhaps to both at once, it occurred that this was simply an example of resonance, the sodium vapour taking up, or absorbing, all the energy of the forced vibration of its own natural

period. The Fraunhofer dark D line in the solar spectrum is thus accounted for by absorption of light by sodium vapour in the sun's atmosphere.

In after years, Thomson never failed to mention in his class this "elementary principle of spectrum analysis", as he called it, and he always said that he had learned it from Stokes.

Towards middle life, Stokes began to devote himself more to public duties, and his scientific activity slackened. For thirty-one years, from 1854 onwards, he was secretary of the Royal Society. In 1885 he succeeded Huxley as President of the Royal Society, holding this office till 1890, when he was himself succeeded by Lord Kelvin. He was Conservative member of parliament for Cambridge University from 1887 to 1891, and in 1889 received the honour of a baronetcy. He died in 1903.

Ludwig von Helmholtz.

One of Thomson's greatest friends was the celebrated German scientist, Ludwig von Helmholtz. They first met in 1855, when the Thomsons were on a visit to Creuznach, for the sake of Mrs. Thomson's health. Helmholtz was then thirty-four. He had begun life as an army surgeon, but quickly drifted into science and at this time was on the point of vacating the chair of physiology at Königsberg for the similar chair at Bonn.

Thomson had read Helmholtz's famous memoir of 1847 on *The Conservation of Force*, while Helmholtz on his side had published a paper on Thomson's thermodynamical work, so that they were delighted to meet each other. Helmholtz wrote to his wife that he was astonished to find one of the first mathematical physicists in Europe so young a man, and expressed the opinion

that Thomson far exceeded all the men of science he had ever met, in intelligence, and lucidity, and mobility of thought.

In future years Helmholtz stayed with the Thomsons, in Arran or in Glasgow, on many occasions. Once in the early days of the gyrostats, Thomson was showing him how he set a heavy metal disc rotating at great speed, when something went wrong and a heavy flying piece of metal just missed Helmholtz's head, but irretrievably ruined his hat!

On one of the visits to Glasgow, Helmholtz met Professor James Thomson, Kelvin's elder brother. He wrote to Frau von Helmholtz that the two brothers talked at each other with great vivacity all the time, but on entirely different subjects; James, however, was the more persistent, and generally managed to get the last word. Helmholtz had a high opinion of James, and was one of the five or six great scientists whose testimonials helped to secure for him the chair of engineering at Glasgow in 1873.

In 1871, the great treatise of Thomson and Tait was translated into German by Helmholtz and a collaborator. In his preface Helmholtz makes this reference to the authors: "One of the authors, Sir Wm. Thomson, has long been known in Germany as one of the most penetrating and ingenious of thinkers who have applied themselves to our science. When such a one undertakes to lead us, as it were into the workshop of his thoughts, and to reveal the way in which he looks at things, . . . we can but feel towards him the liveliest gratitude. For this work, which would indeed overstrain the powers of a single much occupied man, he has found in P. G. Tait, professor of natural philosophy in Edinburgh,

a highly fit and gifted collaborator. Only perhaps by such a happy union could the task as a whole have been completed."

Through Thomson, Helmholtz became acquainted with Tait, and one summer spent a holiday with him at St. Andrews. To Tait this meant golfing, to which, so far as he was concerned, every other form of recreation had to give way. His son, lieutenant in the Black Watch and killed in the Boer War, was the best amateur golfer of his time, and everywhere affectionately known as "Freddie" Tait. Helmholtz could make nothing of the game. He wrote to his wife that though Tait could hit the ball every time, he himself as a rule hit either the ground or the void. Nothing, he remarked, could be got out of Tait on weekdays on any subject but golf; on Sundays, he could not play, and he did not go to church, so they had on that day some opportunity for a little rational conversation.

Helmholtz came to be recognized as one of the very greatest mathematical physicists of the nineteenth century. In physiological optics and acoustics his work was also of the first rank. That very important instrument, the ophthalmoscope, will always be associated with his name.

From 1871 he held the highest posts in Germany open to a physicist, and he might have been, if he had chosen, the first holder of the Cavendish chair of physics at Cambridge. Among his pupils was Heinrich Hertz.

When Helmholtz died in 1894, the loss to Kelvin was very severe. They had been devoted friends for nearly forty years.

Peter Guthrie Tait.

Of Tait something has already been said in connexion with the great work of which he was joint author with Thomson (Chap. VI). After taking his degree as senior wrangler and winning the first Smith's prize in 1852, he was elected a Fellow of Peterhouse, and remained at Cambridge for two years. In 1854 he was appointed professor of mathematics in Queen's College, Belfast, and in 1860 came to Edinburgh, where he remained as professor of natural philosophy for the rest of his life, and earned the reputation of being the best lecturer of his time. As holders of similar chairs in the two largest universities of Scotland, Thomson and Tait were bound to come into contact at many points, but the chief link between them, apart from their great work, was their common interest in the Royal Society of Edinburgh, of which Tait was secretary for many years, and Kelvin was President again and again.

Kelvin often urged Tait to become a Fellow of the greater Royal Society in London, but Tait steadfastly refused. There is a well-known and true story that once, about 1880, when Tait was directly approached from London, just after an able and valued friend of his had been rejected, he replied that he had no pretensions to belong to a society which was too good for his friend. The truth was, however, that for the last twenty years of his life, and more, Tait was practically a recluse, never leaving Edinburgh except for a golfing holiday.

When Tait died, in 1901, his obituary notice in the proceedings of the Royal Society of Edinburgh was prepared by Lord Kelvin, who wrote: ". . . I found him full of reverence for Andrews and Hamilton, and

enthusiasm for science. Nothing else worth living for, he said; with heart-felt sincerity, I believe, though his life belied the saying, as no one was ever more thorough in public duty or more devoted to family and friends. His two years as don of Peterhouse and six of professorial gravity in Belfast had not polished down the rough gaiety nor dulled in the slightest degree the cheerful gaiety of his student days; and this was a large factor in the success of our alliance for heavy work, in which we persevered for eighteen years. 'A merry heart goes all the day, Your sad, tires in a mile-a.' The making of the first part of T and T′ was treated as a perpetual joke, in respect to the irksome details of interchange of copy, amendments in type and final corrections of proofs. It was lightened by interchange of visits between Greenhill Gardens, or Drummond Place, or George Square, and Largs or Arran, or the old or new college at Glasgow; but of necessity it was largely carried on by post. . . .

"After enjoying eighteen years' joint work with Tait on one book, twenty-three years without this tie have given me undiminished pleasure in all my intercourse with him. I cannot say that our meetings were never unruffled. We had keen differences (much more frequent agreements) on every conceivable subject—quaternions, energy, the daily news, politics, &c. We never agreed to differ, we always fought it out. But it was almost as great a pleasure to fight with Tait as to agree with him. His death is a loss to me which cannot, as long as I live, be replaced.

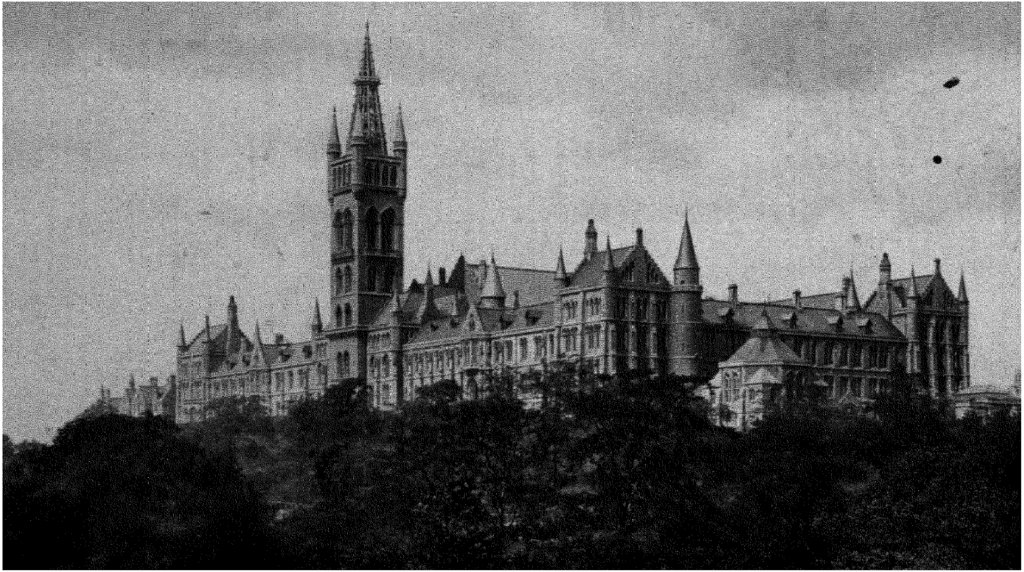
"The cheerful brightness which I found on our first acquaintance forty-one years ago remained fresh during all these years, till first clouded when news came of the

death in battle of his son Freddie in South Africa, on the day of his return to duty after recovering from wounds received at Magersfontein. The cheerfulness never quite returned."

James Clerk Maxwell.

Clerk Maxwell was born in Edinburgh in 1831, the same year as Tait, and, like Tait, received part of his education at Edinburgh Academy and Edinburgh University. He graduated B.A. at Cambridge as second wrangler in 1854, the senior being Routh, the famous coach; the two were equal for the Smith's prizes. It is rather remarkable that of the five great British mathematical physicists of the nineteenth century—Stokes, Thomson, Tait, Maxwell, Rayleigh—the two who failed to get the senior wranglership were the two whom most people would consider to be the greatest of them all. Certainly for sheer physical insight Maxwell, if surpassed at all, was only surpassed by Thomson himself.

While still an undergraduate, Maxwell made the acquaintance of Thomson, who was a frequent visitor to Cambridge in the early years of his professorship. After taking his degree Maxwell wrote to Thomson about his studies in electricity, and received a helpful reply. In after years, Maxwell more than once referred to Faraday and Thomson as the chief inspirers of his work. Indeed, it was a paper of Thomson's published in the Cambridge and Dublin Mathematical Journal in 1847 and entitled "On a mechanical representation of electric, magnetic and galvanic forces", which started him on the researches which culminated in his epoch-making electromagnetic theory of light.



The New University Buildings, Gilmorehill, Glasgow

After holding professorships in Aberdeen and London, Maxwell retired to his country home in Kirkcudbrightshire in 1865, and for six years devoted himself to writing his great treatise on *Electricity and Magnetism*. In 1871 he became the first Cavendish professor of experimental physics at Cambridge, and the world-famous physical laboratory associated with the chair was organized by him. It is interesting to note that on the first three occasions when an appointment was made to this chair, Thomson was offered, or practically offered, the position, but he always declined to leave Glasgow.

Maxwell's famous paper on the electromagnetic theory of light was published in 1864, and encountered much opposition at first, but within twenty years nearly all the leading British physicists had accepted it, and were testing its applications. Kelvin, however, never whole-heartedly accepted the theory, though he more than once referred to it in appreciative terms. In a preface which he contributed to a translation of Hertz's *Electric Waves*, he remarked that " for electricity and magnetism Faraday's anticipations and Clerk Maxwell's splendidly developed theory have been established on the sure basis of experiment by Hertz's work ".

In his Baltimore lectures, Kelvin had said that Maxwell's theory " hitherto has not helped us "; and in his presidential address to the Royal Society in 1893 he indicated some of the reasons for his dissatisfaction. He said: " But splendid as this consummation is, we must not fold our hands and think or say there are no more worlds to conquer for electrical science. We do know something now of magnetic waves. We know

that they exist in nature and that they are in perfect accord with Maxwell's beautiful theory. But this theory tells us nothing of the actual motions of matter constituting a magnetic wave. . . . We have as yet absolutely no guidance towards any understanding or imagining of the relation between this simple and definite alternating motion, or any other motion or displacement of the æther, and the earliest known phenomena of electricity and magnetism—the electrification of matter and the attractions and repulsions of electrified bodies; . . . and certainly we are quite as far from the clue to explaining, by æther or otherwise, the enormously greater forces of attraction and repulsion now so well-known after the modern discovery of electro-magnetism."

Maxwell came into close contact with Thomson in the committee appointed by the British Association in the sixties to deal with electrical standards. One of the reports contains a famous appendix by Maxwell on the Theory of Dimensions as applied to electrical magnitude. Throughout the protracted labours of this committee, which established the absolute system of electrical units practically in their present form, Thomson was the inspiring force, and he was ably assisted by Maxwell.

By his untimely death in 1879, the world of physics lost one of its brightest ornaments. As Newton said of Roger Cotes: "If he had lived, we would have known something."

Lord Rayleigh.

The youngest of the "big five" among British natural philosophers of the nineteenth century, men whose mathematical skill was on the same high level as their

physical insight, was John William Strutt, 3rd Baron Rayleigh.

His career as a student at Cambridge was of the highest possible distinction. His great treatise on the Theory of Sound was published in 1877. On the death of Maxwell in 1879, he became Cavendish professor of experimental physics at Cambridge, retiring in 1884.

He became widely known to the general public by his discovery of argon in 1894. In Kelvin's later years, he leaned strongly upon Rayleigh. When any difficulty arose on which at an earlier period he would naturally have consulted Stokes, it was, from the eighties onwards, just as often Rayleigh to whom he turned for information or opinion.

After Kelvin's death in 1907, Rayleigh was universally recognized as the leader of British science on the physical side.

Thomas Henry Huxley.

The famous biologist, T. H. Huxley, came chiefly into contact with Kelvin in connexion with the great dispute about the age of the earth (see Chap. X). Huxley had at an early age established himself as one of the leading authorities in natural science. He was only twenty-six years of age when he was admitted a Fellow of the Royal Society in 1851, the same year as Thomson and Stokes. His celebrated encounter with Bishop Wilberforce at the British Association meeting at Oxford in 1860 had secured his reputation as a keen controversialist and an unsurpassed master of dialectic, so that it was only to be expected that he would be the one to come forward as the protagonist of the geologists when, in the late sixties, Thomson challenged the

current theory of geophysical uniformity and the unlimited demands for time which it involved.

In his presidential address to the Geological Society of London in 1869, Huxley appealed from the physicists to " that higher court of scientific opinion to which we are all amenable ". In a passage often quoted, he sought to minimize Thomson's mathematical arguments: " I desire to point out that this seems to be one of the many cases in which the admitted accuracy of mathematical processes is allowed to throw a wholly inadmissible appearance of authority over the results obtained by them. Mathematics may be compared to a mill of exquisite workmanship, which grinds you stuff of any degree of fineness; but, nevertheless, what you get out depends on what you put in; and as the grandest mill in the world will not extract wheat-flour from peascods, so pages of formulæ will not get a definite result out of loose data."

Huxley concluded by claiming that the case against them had entirely broken down, and that it had been shown that they had " exercised a wise discrimination in declining to meddle with our foundations at the bidding of the first passer-by who fancies our house is not so well-built as it might be ".

Thomson in his reply receded not an inch from his ground, and met Huxley's rather scornful attitude with the dignified words: " For myself, I am anxious to be regarded by geologists, not as a mere passer-by, but as one constantly interested in their grand subject, and anxious in any way, however slight, to assist them in their search for truth."

That no rancour was left in the minds of these two great men was amply shown by their subse-

quent references to each other on important occasions.

Huxley's turn came first. In 1871, at the meeting of the British Association in Edinburgh, Huxley was the retiring President, and Thomson was his successor, in introducing whom he paid a generous tribute of admiration and appreciation of his great gifts and achievements, concluding: "Those are the public, notorious and obvious feats of your President-elect. What is less known and less obvious—his personal qualities—are such as I dare not and will not here dwell upon; but upon one matter which lies within my own personal knowledge I may be permitted to say of him, as the old poet says of Lancelot, that

> 'gentler knight
> There never brake a lance'."

In 1883 it fell to Huxley's lot as President of the Royal Society to present Sir William with the Copley medal, the highest honour at the disposal of the society, and he again spoke in most eulogistic terms of Sir William's services to pure and applied science.

In 1894 Thomson, now Lord Kelvin, in turn was President, and in presenting the Darwin medal to Huxley, spoke of his bold, unwearied exposition and defence of the theory of "Natural Selection"; and suggested the possibility that the theory might not have met with the acceptance with which it had met, nor gained the power which it had gained, had it not been for the brilliant advocacy with which in its early days it was expounded to all classes of men.

In 1895 again, Lord Kelvin being still President of the Royal Society, it fell to him in his annual address

to refer to the death of Huxley. He remarked that everywhere in Huxley's writings "we find traces of acute and profound philosophic thought. When he introduced the word agnostic to describe his own feeling with reference to the origin and continuance of life, he confessed himself to be in the presence of mysteries on which science had not been strong enough to enlighten us; and he chose the word wisely and well. It is a word which, even though negative in character, may be helpful to all philosophers and theologians. If religion means strenuousness in doing right and trying to do right, who has earned the title of a religious man better than Huxley?"

Chapter XII

LORD KELVIN'S ASSISTANTS. PATENTS

James Thomson Bottomley (1845–1926).

FOR nearly thirty years Kelvin's nephew, J. T. Bottomley, acted as his assistant and deputy for much of the routine work of the chair of natural philosophy at Glasgow. Bottomley was born in Belfast, his mother, Anna Thomson, being the second of Lord Kelvin's sisters. His early education was mainly private. After a brief attendance at Queen's College, Belfast, he went to Trinity College, Dublin, where he graduated with distinction. His first post was that of assistant to Prof. Thomas Andrews, F.R.S. of Belfast, and he then went to King's College, London, where he gained some experience as a demonstrator, first in chemistry and then in physics. In 1870 he came to Glasgow as private assistant to his uncle, then Sir William Thomson, and five years later when the Arnott and Thomson Demonstratorship in Experimental Physics was founded, he was appointed the first demonstrator. In these two capacities, Bottomley was responsible for a very considerable share of the work of the Natural Philosophy classes. In his later years Sir William Thomson generally lectured twice a week and the remainder of the lectures were given by Bottomley. He held this post until Kelvin resigned in 1899.

During the years of his connexion with Glasgow

University, Bottomley was continually engaged in very varied research work. He was interested in the liquefaction of gases and followed up some of Andrews' researches. He devised the well-known experiment of a wire slowly cutting through a block of ice, the sides of the cut freezing together after the wire has passed. This is an excellent illustration of the theoretical prediction of his uncle, Professor James Thomson—Lord Kelvin's brother—of the lowering of the temperature of the freezing-point of water by pressure.

Bottomley made independent researches on the use of liquid air for experiments on radiation at very low temperatures, on the air thermometer, on the thermal emissivity and conductivity of wires in a vacuum, on radiation from bright and black bodies and on vacuum pumps, as well as many other researches in conjunction with Lord Kelvin. Many of his results are published in the proceedings of the Royal Society and in the British Association Reports.

He did good work, too, in connexion with electrical engineering. Probably the first paper ever published on the efficiency of the electric incandescent lamp was communicated to the British Association by Kelvin and Bottomley in 1881 (B.A. Report, p. 559).

The potential difference required for the experiments was got from Faure accumulators. A galvanometer was used to measure this, while another measured the current. The former they called the potential galvanometer, and the latter the current galvanometer. The illuminating power of the lamps was measured by Rumford's method, a pencil being used to cast the shadows, the intensities of which had to be compared. They considered it unnecessary to weigh the standard

candle before and after the experiment, and assumed that it was burning away at the rate of 120 grains of wax per hour, pointing out that both voltage and life tests must be applied before the most economical working voltage can be determined.

In 1879 the author remembers Dr. Bottomley as a painstaking and most amiable teacher. As an experimenter he was very good, being neat handed and clever at arranging apparatus. He was most skilful at glass blowing.

Dr. Bottomley was married twice. His first wife was Annie, daughter of W. Heap of Manchester; his second, Mrs. Duff, née Blandy, who was the sister of Lord Kelvin's second wife. She died in 1913.

He joined the business of Kelvin, Bottomley and Baird when the firm was started as a private company in 1900, and on the death of Lord Kelvin in 1907 was elected chairman of the directors, a post which he held until his death. After Lady Kelvin died he lived in Kelvin's old house, Netherhall, Largs, but in 1926, having become partially blind, he retired to his house at University Gardens, Glasgow, where he died on 18th May. He was elected a Fellow of the Royal Society in 1888 and received the honorary degree of LL.D. from the University of Glasgow in 1904.

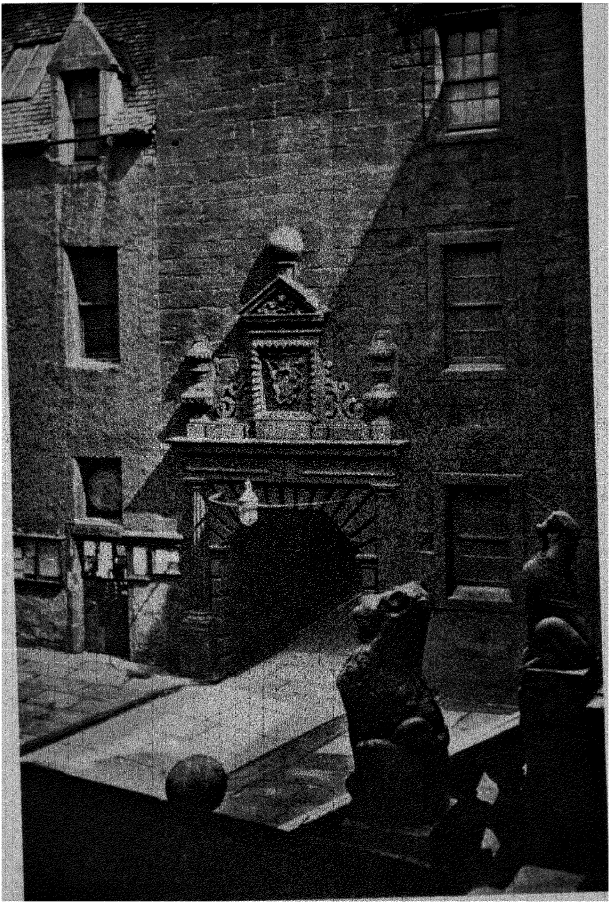
Andrew Gray (1847–1925).

From 1875 to 1880 Andrew Gray was private secretary to Sir William Thomson, and from 1880 to 1884 he was his official assistant. His education was the usual education of a Scottish boy who is the son of parents of limited means. The master at the school he attended laid stress on the practical importance of mathematics. He taught

the boys the elements of surveying, and in particular he showed them how to measure the distance of conspicuous objects out of doors by means of a measured base line. In this way Gray when a boy measured the distance of Nelson's monument on the Calton Hill, the lighthouse on the island of Inchkeith, the Martello tower at Leith harbour, North Berwick Law, and other objects that can be seen from Burntisland.

At Glasgow University he gained many prizes and graduated M.A., with honours in mathematics and natural philosophy. His university distinctions, however, inadequately represent his attainments. On more than one occasion, when a student, he had to leave the university a few weeks before the end of the session in order to assist in farming operations at home. In the opinion of Dr. Lushington, the Professor of Greek, this lost him the gold medal which is given to the best student in the senior Greek class. Most students would have been grievously disappointed at losing such eagerly coveted honours, but Gray always put duty before personal ambition, and did not seem to mind. To the end of his life he remained an excellent classical scholar, and had a keen appreciation of Greek and Latin poetry. His Greek testament was his constant companion, and in his letters he not infrequently made apt quotations from it. Like his friend Professor Chrystal, he was an admirer of Schiller's poems, and knew many of them by heart.

From 1875 to 1884, when he was Thomson's assistant, Gray had no easy task. About this time electrical engineering was making great strides, and Thomson had a devoted band of workers in his laboratory. Gray took a leading part in the testing of dynamos, at one



A GLIMPSE OF THE COURTYARD OF OLD COLLEGE, GLASGOW

The staircase (from which the view was taken) has been re-erected in the new College at Gilmorehill

time in conjunction with Dr. John Hopkinson, and in testing accumulators and electric lamps.

In 1881–2, when the author attended Sir William Thomson's Senior Natural Philosophy class, Andrew Gray used sometimes to sit with the students and take a few notes. At the end of the term he corrected the examination papers of the class and returned them to the students. The author found that not only had he written out the solutions of those questions which had been answered incorrectly, but also he had written out complete solutions of all those which had not been attempted. It must have taken him an hour or two, and shows the interest which he took in students and how much he liked to help them.

In 1884 Gray was appointed to the chair of Physics in the newly founded University College of North Wales, Bangor. He had as colleagues two other old Glasgow students: the distinguished philosopher, subsequently Sir Henry Jones, Professor of Moral Philosophy, who had been Gray's colleague at Glasgow University; and James J. Dobbie, afterwards Sir J. J. Dobbie, the Government chemist. In 1896 Gray was made a Fellow of the Royal Society and received the Hon. degree of LL.D. from Glasgow University. While in Wales, he championed the cause of the higher education of women and took a leading part in the foundation of the County School for girls at Bangor. At this time he was also an enthusiastic mountaineer, and made weekly excursions with some of his colleagues into the Welsh hills. He was a strong swimmer and rarely missed his morning bathe in the Menai Straits.

In 1899 he was installed Kelvin's successor as Professor of Natural Philosophy at Glasgow University,

and retained the chair until in 1924 ill-health forced him to resign the post. During this period his strong personality, ability as a teacher and unwearying patience in explaining difficulties endeared him to students. On the death of Lord Kelvin in 1907, Gray delivered an eloquent oration on the life and work of his former chief. He later expanded this into a book called *The Scientific Work of Lord Kelvin*, which gives an excellent account of the life and activities of his famous predecessor.

Gray planned the present Natural Philosophy Institute of the University, a task which absorbed all his energies for many years. He paid special attention to the methods of ventilating and keeping at an equable temperature the large lecture rooms and laboratories and did everything possible for the comfort of the students. He also arranged a collection of Kelvin's apparatus in the Institute, a collection still recalling to old students the exciting times in the laboratory when some new instrument received its final touches, not the least excited among us being Kelvin himself.

Gray wrote many books, published several addresses and communicated many papers to the Royal Societies of London and Edinburgh. His first book entitled *Absolute Measurements in Electricity and Magnetism* was published in 1883. It deals almost exclusively with work of a fundamental character which was being carried out at that time by workers assisting Sir William Thomson. The first volume of an expansion of this work appeared in 1888 and a second volume in 1893. These volumes proved most helpful to physicists in national laboratories when determining our electrical standards. In 1921 a final edition of *Absolute Measure-*

ments appeared. He once said that the writing of this book was difficult owing to—if not the discouraging—at least the Laodicean attitude of scientists to absolute measurements. In 1895 he published in conjunction with George Ballard Matthews a *Treatise on Bessel Functions*, a work in which practical applications are given particular attention. This treatise, which became a classic, was revised by Prof. Gray and Prof. T. M. MacRobert in 1922.

In 1898 appeared a treatise on *Magnetism and Electricity*, and in 1901 a volume on *Dynamics and Properties of Matter*. In 1911, in conjunction with his son, Prof. J. G. Gray, he published a treatise on *Dynamics*, for students of physics and engineering, containing a very large number of interesting examples, complete solutions of many of which are given. In 1919 appeared *A Treatise on Gyrostatics and Rotational Motion*. This book is a monument to the vigour and industry of the author, as well as to his thorough knowledge of the subject.

In 1912 the author wrote to Gray to ascertain his opinion of the proposed use of the word "Kelvin" to denote the practical unit of electrical energy. In particular he was asked what he thought Lord Kelvin's feeling would be likely to be if he were alive. He replied: "I have no hesitation in saying that I think the idea would not be distasteful to him, but the contrary. That view I base on my general reading of Lord Kelvin's character and disposition. He certainly did like recognition and appreciation (no blame to him for that!), and I am sure that any legitimate mode of perpetuating his name and fame which his colleague-workers in electrical science had insisted on inaugurating would have been gratefully received during his lifetime."

Personally, however, Gray was strongly opposed to the giving of the names of eminent scientific men, *nomina clara et memorabilia*, as designations for practical electrical units. On another occasion, when criticizing parts of *Thomson and Tait*, he wrote: " You will understand that I yield to no one in respect for the genius and memory of my great teacher and predecessor."

He was much interested in Einstein's theory and had intended to give lectures on the subject. He thought it a pity that it was generally presented in the form of the " Theory of Tensors ". In a letter written in 1923 he says: " Some of the conclusions of the theory—e.g. as to the magnitude of the universe—are hardly translatable out of the non-Euclidean geometry without a dislocation of our reason. I cannot follow the conclusions . . . by the methods of common sense, if such methods be applicable. They almost seem to outrage all our old ideas." The Einsteinians were very sane people, but it was difficult to reconcile some of their results with sanity. " I don't know what to think!"

Prof. Gray was very happy in his home life. There was an interesting family gathering when he and his wife celebrated their golden wedding. In his later years he loved to spend his vacation in the Perthshire Highlands, where golden eagles are sometimes seen. His was a happy, kindly and active life.

Donald McFarlane.

A few years after Thomson had begun lecturing, he found it necessary to have an assistant to help him with his work, and Mr. Donald McFarlane, who had distinguished himself in the mathematical and natural

philosophy classes, was appointed. All who ever attended the natural philosophy class will remember him, as he was a great favourite with the students. Professor Andrew Gray, who knew him well, says that he was originally a block-printer and seems to have been an apprentice at Alexandria, in the Vale of Leven, at the time of the passing of the first Reform Bill. After some time spent in the cotton industry of the district, he became a teacher in a village school and afterwards entered the university as a student.

He was a most conscientious assistant to Thomson. He had charge of the instruments of the department, got ready the lecture illustrations, and attended during lectures to assist in the experiments and to supply numerical data when required. McFarlane prepared the weekly examination paper held on Mondays, corrected all the papers sent in, kept a record of all the marks, and on Friday afternoon at 3 o'clock gave the solutions of all the questions set on the Monday to any of the students who cared to attend. His clear explanations were much appreciated by them, and at the end of every year the class used to present him with some valuable gift to which all had subscribed.

Besides taking part in the teaching work he assisted in the original researches which the professor was always ardently pursuing. He was a very expert calculator as well as a very exact and careful experimentalist. Some of the diagrams he drew and the tables he constructed are still copied in standard textbooks. Then in his spare time in the apparatus room McFarlane would sit down to correct the huge pile of Monday's examination papers.

Professor A. Gray says that he was a man of the

highest ability and of the most absolute unselfishness. Very few if any of the class knew where he lived. It was believed that the only recreation of his solitary life was an hour in the evening with one or two friends, and the study of German. He was full of humour, and told with keen enjoyment stories of the University worthies of a bygone age. For years he lived on a meagre salary, but in 1861 an extra Government grant of £100 per annum was given him by the University Commissioners. He had a boundless admiration and veneration for Thomson and was prepared to do anything he asked him to do. After his retirement in 1880 he lived in Glasgow, amusing himself by reading out-of-the-way literature and making abstruse calculations of eclipses. He finally returned to Alexandria, where he died in February, 1897. He was remembered with affection by all his old students.

Magnus Maclean (1858–1937).

Magnus Maclean was born in Skye in 1858 and received his education at the General Assembly School in Colbost, latterly as a pupil teacher. On gaining a Queen's scholarship he entered the Free Church Training College in Glasgow, attending some lectures at the University, and at the end of his training became a teacher in a school in Sutherlandshire. Gaining a Highland Society bursary, he re-entered Glasgow University, where he obtained a Lorimer bursary in mathematics and a Thomson experimental scholarship in the physical laboratory attached to the natural philosophy department. His work in the laboratory was the beginning of a close association with Kelvin which continued for many years. After graduating as

M.A., with honours in mathematics and natural philosophy, he became Lord Kelvin's chief assistant. In 1892 he was appointed lecturer in physics to medical students, and three years later became lecturer to the engineering students in pure and applied electricity. In recognition of his research work and the papers he had contributed to learned societies, the University conferred on him the degree of D.Sc. and he was elected a Fellow of the Royal Society of Edinburgh.

When the chair of electrical engineering in the Royal Technical College, Glasgow, became vacant in 1899, he was appointed professor; a position which he held for twenty-five years. In 1903 he was chosen as a member of the Moseley commission on education, and with the commission visited the United States.

Professor Magnus Maclean was also a well-known authority on the Gaelic language and literature. In 1919 Glasgow University conferred on him the honorary degree of LL.D. for his distinction in electrical science and as a Gaelic scholar. He retired from the chair in 1923 owing to the age limit. He was the author of important works and textbooks on electrical engineering, and was a member of the Institution of Electrical Engineers, being twice chairman of the Glasgow local section. He died in 1937 in his eightieth year.

James Gordon Gray (1876–1934).

J. G. Gray was the second son of Professor Andrew Gray and was born at Bangor, N. Wales, in 1876. He was educated at Friars School and at the University of N. Wales. As a student he was an athlete, a great football player and a strong swimmer. He studied at Glasgow University, distinguishing himself in the

mathematics and natural philosophy classes, and afterwards joined the staff of the General Post Office, working under Sir William Preece. Returning to Glasgow, he took his B.Sc. degree in 1901. The University conferred on him the degree of D.Sc. for his research work, and in 1903 he became university lecturer in natural philosophy. In 1920 he was appointed Cargill Professor of Natural Philosophy at Glasgow. In conjunction with his father he wrote an excellent treatise on *Dynamics*, freely introducing arithmetical questions in connexion with the applications of gyrostatic principles to practical problems. He took out many patents and greatly forwarded the applications of gyroscopes in aerial and marine navigation and in national defence. He gave many lectures on gyroscopes at the Royal Institution, Society of Arts, Institution of Electrical Engineers, &c., and was a leading authority on this subject, following in the footsteps of Lord Kelvin and of his father. He died in 1934.

George Green (1881–   ).

George Green, born in September, 1881, a university lecturer on natural philosophy at Glasgow, was Kelvin's last personal assistant before his death. Green has written many papers on problems connected with the conduction of heat, and has obtained valuable results by using Bessel's functions in novel ways to give solutions the numerical values of which are easily found. His connexion with Lord Kelvin began in November, 1905, and continued till his death in December, 1907—just over two years. In Kelvin's ordering of the day during this period, higher scientific research and investigation came second and not first. The time up to about 3 p.m.

every day, and not infrequently the whole day, was spent in work which he referred to as "earning our bread". This work related chiefly to tests of instruments and to experiments going on at the works of Messrs. Kelvin and James White, Ltd. When Green became secretary, the sounding machine had just taken final shape, but a long series of experiments followed in search of a less exposed method of sounding, suitable for battleships; these experiments were carried out at the Admiralty. The problem of projecting an image of the compass card up to the steersman in cases where his position was one unsuitable for the compass itself also came up for solution.

In addition to the more or less regular work for Messrs. Kelvin and James White, Ltd., Green can remember writing a report on turbines for the British Westinghouse Company, doing calculations for the Victoria Falls Power Company, and carrying out some preliminary experiments for the Dunderland Iron Ore Company.

No matter how keenly he might be engaged on the more difficult and perhaps more congenial theoretical work of his scientific papers, Lord Kelvin could—and always did—lay it aside when his help was required to obtain some "good and useful practical result".

On the theoretical side of Kelvin's work, three papers were published in the two years of Green's secretaryship. Doubtless information could be obtained from the famous "green books" regarding other subjects which occupied his mind. These green books were all sent to Professor Larmor after Kelvin's death and have been published.

Kelvin was keenly interested at this period in "atomic

electrostatics", trying to find an atomic explanation of the spectral lines of hydrogen, &c. He was also trying to explain the conductivity of metals by electrons. Green says that the field of investigation represented by the work of Born on crystals, &c., was certainly opened up by Lord Kelvin. He was also looking for a solution for the motion of an electrion (electron) based on the theory of an elastic solid; and for an explanation of magnetism on the hypothesis that it was due to some form of rotation in the æther.

Lord Kelvin's Patents.

Lord Kelvin's first patent was taken out in 1854, and his last in 1907. In all he took out seventy, eighteen of them in conjunction with other inventors. Thirteen of the fifteen taken out between 1902 and 1907 were in the name of Lord Kelvin and of the firm Kelvin and James White. Those bearing the name of the firm related to compasses and sounding machines.

The first patent taken out in 1854 described improvements in electrical conductors for telegraphic communication, and was taken out in conjunction with W. J. Macquorn Rankine, who in the following year was appointed Professor of Civil Engineering in Glasgow University, and with his brother James Thomson who was Rankine's successor. With Fleeming Jenkin he had one in 1860 and another in 1873, both referring to improvements in telegraphic apparatus. In 1865 he took out another patent on this subject with C. F. Varley. In 1884 in conjunction with S. Z. de Ferranti he patented improvements on dynamo-electric machines. In the same year in conjunction with J. T. Bottomley he took out a patent for safety fuses in electric cir-

cuits. All other patents were in his own name solely.

The full titles and numbers of these are given in Silvanus Thomson's *Life of Lord Kelvin*, Vol. II, Appendix C. In a paper read before the Philosophical Society of Glasgow in February, 1898, Magnus Maclean gives a list of them from 1858.

He classifies them under four heads:

(*a*) Patents relating to improvements in electric telegraphic apparatus.

(*b*) Patents relating to improvements in navigational apparatus.

(*c*) Patents relating to improvements in generating, regulating, measuring, recording and integrating electric currents.

(*d*) Patents relating to improvements in valves for fluids.

Under the first heading (*a*) there are 11 patents, containing 211 pages of descriptive reading and 24 large sheets covered with 127 separate figures. Under the second heading (*b*) there are 23 patents.

Under the third heading (*c*) there are 24, containing 177 pages of descriptive reading, and 123 sheets with 287 figures or diagrams.

Under the fourth heading (*d*) there are only two items, which relate to improvements in valves for water, steam, or other liquids or gases. Up to September, 1896, there were 47 patents, containing 487 pages of descriptive reading and 182 sheets having at least 592 separate figures.

Under the first heading (*a*) reference is made in the patents to the " retardation " of the signals caused by " electrostatic capacity " between cables, a subject which

at that time was practically unknown, and emphasis was laid on the necessity of having a receiving instrument so sensitive that it could record continuously every variation in the strength of the received current. Both the mirror galvanometer and the siphon recorder which are briefly described fulfil these conditions. Under the second heading, the sounding machine and the new mariner's compass are the best known.

An account of the electrometers and electrostatic instruments described in the patents under heading (*c*) is given in Thomson's Reprint of Papers on *Electrostatics and Magnetism*. These instruments read correctly with both direct and alternating currents. In conjunction with accurate resistances they give an excellent method of measuring amperes and watts as well as volts.

Thomson's electric balances for measuring currents were made in five sizes, from the centiampere to the kiloampere balance. The principle on which they work depends on the mutual forces discovered by Ampère between the movable and fixed portions of an electric circuit.

CHAPTER XIII

LAST YEARS

ON Kelvin's retirement in 1899, he left Glasgow and went to live at Netherhall, the comfortable homely mansion at Largs he had planned for his holiday home. He loved his garden and he loved having friends staying with him, although he never allowed them to interfere with his research work but left them to his wife to entertain. Fortunately she was an excellent hostess and took a real pleasure in their society. The railway at Wemyss Bay was quite near, giving a quick run into Glasgow.

Kelvin was very fond of pets, particularly of parrots, and he had a horror of any unnecessary slaughter. Once on the *Lalla Rookh* he was intensely annoyed when a friend attempted to shoot a seagull. He thought vivisection was justifiable if necessary to save human life or suffering, but the cause must be undoubtedly adequate. He regarded war as barbarism, a mere relic of savagery, and believed that like duelling it was destined to die out. He wrote to Tait in 1879: " It must not be supposed that those who do not want to fight are devoid of patriotism."

He took part in the modification of the Cambridge Mathematical Tripos which added heat, electricity and magnetism to the curriculum, but wrote to Routh in 1906 that he hoped " the order of merit in the Mathe-

matical Tripos will be maintained. The Senior Wranglership is an institution at Cambridge which ought not to be abandoned. I believe its maintenance has a beneficial effect on the whole Tripos." Many would have agreed with him.

Kelvin made no parade of religious views, but he had thought things out for himself at an early age and possessed a faith which not having been received second hand was never shaken. He was simple and child-like and completely undogmatic, with no bigotry or intolerance. Although brought up in the Church of Scotland, at Cambridge he conformed to the Church of England. He had two hatreds, one of sacerdotalism and ritualism—the High Church in his opinion was high only in the sense that game is high——; the other of Spiritualism, which he went out of his way to denounce as "that wretched superstition fostered by imposture". One of his friends writes of him, "he was a sincere Christian as taught by Christ and not by the churches, and looked deep into essentials; he regarded differences of sects as mere matters of form and looked on the distinctions between Episcopalians, Presbyterians, Quakers and Unitarians with supreme indifference".

Kelvin was fond of novels which bore on sea-going life; he was devoted to the sea, and sailing was his greatest pleasure. He loved the sunshine and would never allow a window blind to be lowered. He was very fond of little children, and all his life his love of music was a ruling passion, although he never cared for Wagner whose plots he thought silly. In 1900 he took a London house, 15 Eaton Place, and he and his wife paid an annual visit to Aix les Bains, principally for the sake of her health. In 1902 they both visited

America, a most enjoyable visit although it lasted for only three weeks.

He shows his extreme common sense and business mind by his remarks on Niagara. "Beautiful as that wonderful work of nature is, it would be more beautiful still if those waters fell upon turbine wheels, every one of which was turning the wheels of industry."

In January, 1905, Lord Kelvin was seriously unwell, suffering from an obscure form of facial neuralgia, and in the spring he underwent a surgical operation. Worse came in 1906, when his eyesight was threatened and he became blind in one eye from detachment of the retina. In 1907 he looked altered and feeble, and was both lamer and deafer. Still he was fit enough to accompany his wife to Aix les Bains as usual. On their return Lady Kelvin seemed terribly tired on the journey home to Netherhall and collapsed on her arrival there. She became dangerously ill, and for many days was in a most critical condition. Her husband never lost faith in her recovery and worked on as usual, trying to obtain relief and consolation in this way. He himself caught a chill after a drive on the 23rd of November and this led to symptoms of septic fever. Lady Kelvin was by now sufficiently recovered to visit her husband's bedside two or three times, but he never rallied and he died on the 17th December, 1907. He was buried in Westminster Abbey near Newton and Herschell under a slab inscribed:

William Thomson, Lord Kelvin, 1824–1907.

Let us now praise famous men. . . .
Their bodies are buried in peace;
But their name liveth for evermore.

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